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THE SWAN-MUSSEL AND ITS ANATOMY.

BY THE REV. W. HOUGHTON, M.A., F.L.S.

(With a Tinted Plate.)

WHAT collector of shells is unacquainted with the form of the fresh-water mussel, as it lies half buried in the mud, with its back or umbonal region, easily recognized by its white pearly appearance, above the surface; or as the same animal lies flat on the bottom of the river or pond, with its large, broad, and semi-transparent foot protruded between the valves of the shell two inches or more? The swan-mussel (*Anodonta*), of which the most recent English authority* recognizes two British species, the *Anodonta cygnea* and the *Anodonta anatina*, is one of the largest of our bivalve molluscs, and its study a matter of curious and more than ordinary interest, especially in that portion of its history which relates to the development of the young fry.

The *Anodonta*,† so named from the absence or very rudimentary condition of the teeth of the *hinge*, belongs to the family *Unionidæ*, a group of the *Conchifera*, or Bivalves, of the order *Lamellibranchiata*, the individuals of which are characterized by the possession of four *branchiæ* or gills arranged on each side of the body in pairs. The *Lamellibranchiata* are all aquatic animals, and the greater part inhabit the sea. Of the fresh-water kinds, the *Anodonta* and *Unio*, the common round or semi-oval *Cyclas corneum*, and the curious sub-triangular shell of *Dreissena polymorpha*, afford instances of the three native families, the *Unionidæ*, *Sphæriidæ*, and *Dreissenidæ*.

Let us now glance at some of the most important parts of the anatomy of the swan-mussel, which, from the large size of

* Gwyn Jeffreys' *British Conchology*, vol. i.

† From ἀ, "not," and ὀδούς, gen, ὀδόντος, "a tooth."

the animal, is more readily made out than in many other bivalves.

The shell of the *Anodonta cygnea* differs from that of the other *Unionidæ* in the absence of distinct teeth in its hinge; it is oblong in shape, and compressed for the whole space between the hinge and the posterior extremity; in colour it varies much, young specimens being light olive, with dark concentric bands or lines, which are supposed by some to mark the animal's growth; at the region of the *umbo* the shell is always pearly from the erosion of the outer skin or *epidermis;* but what causes the erosion in this particular point it is very difficult to determine;* older specimens are darker in colour; the valves of the shell internally are of a whitish or yellowish pearly lustre, and sometimes very iridescent; the marks of the muscles and mantle, so distinct in some marine Conchifers, are very faint in the *Anodonta.*

The animal that tenants the shell is represented in Figure 2; *d* representing one of the external pair of gills greatly distended with thousands of young Anodons, which bear no resemblance whatever to the parent animal. The Anodon is attached to the shell by two muscles, the anterior and posterior adductor; its parts consist of the mantle, a thin fleshy envelope, which covers the whole body of the animal, and consists of two folds or leaves which are joined at the back part but open in front. M. Moquin-Tandon well compares the disposition of the two lobes of the mantle to the "covers of a book when it is placed on its edge with the back uppermost." Along the margin of the dorsal region the mantle forms a raised seam, or line, with a narrow slit posteriorly. The mantle secretes the shell; it is much thickened at the extreme margin of its free portion.

The organization of a fresh-water mussel, like the *Conchifera* in general, is simple, though there are some points which it is extremely difficult to determine with anything like precision. The Anodon is provided with organs of digestion, circulation, respiration, locomotion, and generation; the nervous system also is well developed.

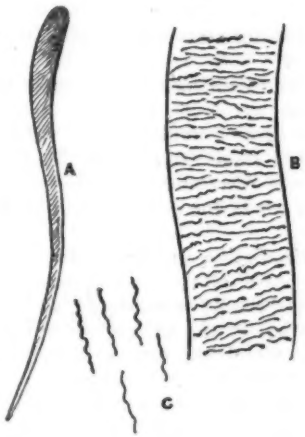
The organs of digestion consist of mouth, stomach, and intestine.† The mouth of an Anodon is a horizontal opening situated between the two pairs of labial palpi, under the anterior juncture of the mantle and just above the foot. The labial palpi are beautifully striated transversely, of a triangularly

* Mr. Jeffreys believes this erosion is caused by the chemical action of gases evolved from the mud in which this portion of the shell is usually imbedded; but the umbonal region is just that part of the shell which is generally *not* imbedded.

† The food of the swan-mussel consists of Desmideæ, Diatomaceæ, and other minute organisms, such as the *spores* of Algæ; this vegetable diet is accompanied with *Rotifera*, minute *Entomostraca*, etc.

oblong form, and furnished exteriorly with a ciliated epithelium, the action of which may be readily seen under a low magnifying lens. Many authors have supposed that these ciliated processes have something to do with the respiration; their more immediate office, however, is to bring particles of food within the reach of the mouth. There does not appear to be any œsophagus in the *Anodonta*, the mouth opening immediately into the stomach, whose internal coat consists of the most delicate lace-like structure; in form it is roundish, and is made up of several smaller depressions, the *biliary cripts* of comparative anatomists. The liver, a large organ occupying a considerable portion of the anterior extremity of the animal, readily recognized by its brownish green colour, consists of a number of *cæca* arranged in racemose clusters, which pour

FIG. 1.



A. Crystalline style of *Anodonta* (natural size).
B. Portion of ditto magnified, showing extremely minute animalcules imbedded in its gelatinous substance.
C. Filiform animalcules, × 400 diam.

their secreted fluid into the stomach and part of the intestine by a number of very minute pores, according to the opinion of the French anatomist * whose anatomical investigations of the molluscs are worthy of the highest consideration. Projecting within the stomach in the *Anodonta* and *Unio*, is a very curious organ whose function has been hitherto unexplained. If you

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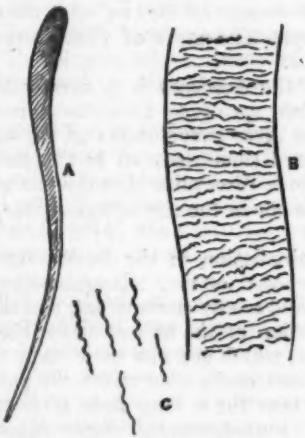

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open with a finely-pointed pair of scissors the stomach of one of these molluscs you will notice a semi-transparent body, of about three inches long in a large animal, and about the thickness of a piece of whipcord, cylindrical and of the consistency of jelly. M. Moquin-Tandon says that this body (called the crystalline style) has been found only in the Anodon and Unio, and does not exist in other acephalous molluscs.

This crystalline style I have invariably found to contain, imbedded within its substance, large numbers of a very minute filiform animalcule, which may be seen under the microscope to shoot backwards and forwards in the gelatinous mass. I have failed hitherto to make out any structure in these curious parasitic vibrioid worms. I have examined them under one of Rosse's $\frac{1}{10}$th of an inch objective, and also under Smith and Beck's $\frac{1}{25}$th, but as I said without the revelation of any structure. These vibrioid animalcules, which occur so constantly in *Anodonta*, are not found, so far as my observations have extended, in the crystalline style of *Unio margaritifera* or in that of *U. pictorum*.

The intestine of the *Anodonta* is a narrow tube with thick jelly-like walls, which, springing from the bottom of the stomach, makes two or three convolutions in the substance of the liver and ovaries, and then rises up to the back of the animal, and passing in a direct line through the ventricle of the heart and under the slit in the rim of the mantle, terminates at the anus.

The organs of circulation in the fresh-water mussels consist of a heart, composed of two triangular-shaped auricles of delicate structure, and nearly transparent, and one ventricle. It lies on the back portion of the animal under the hinge of the shell; its pulsations, which are not more than about seven or eight in a minute, are easily seen under the pericardium: the heart continues to beat for a long time after the valves have been opened. A French *savant* states that after he had placed an *Anodonta* in boiling water to destroy it, and to preserve the shell, the heart actually beat eight hours afterwards! The blood is nearly colourless, and the corpuscles semi-transparent; after traversing the numerous vessels of the gills it is received by the large branchial veins, from whence it is conveyed to the triangular shaped auricles, which have their largest side adjacent to these veins, and their smallest side in communication with the auricles; thence it proceeds to the ventricle, which by its contraction impels the circulating fluid to the aorta and distributes it to the whole system. Underneath the heart, and extending for a considerable distance along the body, is seen a curious sponge-like organ of a chocolate colour, which has received the name of the "gland of Bo-

janus," concerning the function of which organ different opinions have been held. It is probable that this organ, which is double, receives the blood from the veins, and that the greater portion of the circulating fluid finds its way thence to the vessels of the branchiæ, while another portion flows directly to the heart. But the actual course of the blood through the arteries and veins is extremely difficult to study in detail; the main facts, however, as we have stated them, may, we think, be depended upon. It will be seen that one pulse of the aortic ventricle is sufficient to impel the blood through the two systems of vessels and to bring it again to the triangular auricles.

The organs of respiration consist of the two pairs of gills already mentioned; each gill is formed of two membranes, connected together, in the case of the exterior pair, by several transverse septa, forming a number of divisions or pockets (see Fig. 5), which are filled, at certain seasons of the year, with thousands of young fry. Numerous blood-vessels are observed to traverse the substance of the gills, which, being provided with many cilia, serve by their constant action to oxygenate the blood.

The *Anodonta*, like the rest of the *Unionidæ*, moves by means of its long fleshy tongue-shaped foot, which it extends forwards to a considerable length upon the mud, and then making that point a lever, draws itself onwards; its track may often be seen in the mud; the movement is very slow.

If an Anodon be taken out of the water and its valves opened, a quantity of water is seen to issue from the muscular foot. According to the experiments of two recent observers, these animals are enabled thus to distend their foot with water, so that it appears almost transparent, by means of a number of aquiferous canals which traverse the foot, and are everywhere in apposition with the blood-vessels. The water is drawn into the pericardial space, when the bivalve opens its shell, and from thence to the interior of the blood-vessels; from the blood-vessels these authors suppose it to transude into the system of water-tubes, and to find its exit at a number of pores on either side of the foot of the animal. Repeated dissections of these animals have failed to reveal to my own eyes the presence of these foot-pores. Both M. H. Meckel and M. Moquin-Tandon have been equally unsuccessful; but the careful experiments of Dr. Rolleston and M. Robertson seem decisive in favour of their existence.*

With regard to the organs of generation, much remains as yet unexplained. The ascertained facts may be thus briefly

* See *Annals and Magazine of Natural History* for December, 1862.

stated:—The *Anodonta*, like all other Conchifers, is androgynous; that is to say, each individual is both male and female. Numerous authors of considerable authority have maintained the existence of two distinct sexes in the *Lamellibranchiata;* Leuwenhoek, Milne Edwards, Quatrefages, Owen, Van der Hoven, and others, have been of this opinion. "The latest and best observations of naturalists and physiologists," says Professor Owen, "on the sexual character and generation of the *Lamellibranchiata*, have established the correctness of Leuwenhoek's conclusion that these mollusca are of distinct sexes, some individuals being male and others female."* There is no doubt, however, that the Conchifera are androgynous.

The secretory organ occupies a large portion of the animal's body; it is situated behind the liver, and is readily recognized by its yellow colour in *Anodonta*, and by its milky whiteness in *Unio*. If a portion of the ovary be examined under the microscope, it will be seen to consist of a number of small sacs with branching vessels; on the rupture of these sacs a quantity of ova are observed, and surrounding and imbedded in the substance of the ovary, as it appears, there are myriads of oblong bodies of extreme minuteness, which from their motions are commonly regarded as the fertilizing fluid. Neumann has described two oviducts which have their orifice near the orifices of the organ of Bojanus; both which apertures are situated on the upper part of the body, covered by the lamina of the inner gill, at the juncture of the two pairs of branchiæ.† After the expulsion of the eggs from this genital orifice "they are conducted along the base of the interior branchiæ as far as the pallial *cloaca*, and from thence they reascend by the canal which the mantle forms in this place, and arrive at the compartments of the exterior branchiæ. A part of these eggs is sometimes drawn in during the inspiration of the water and expelled as if it issued from the anus—these are soon, however, taken up by the respiratory current and directed towards the branchial pockets." In these compartments the eggs are gradually developed. If an Anodon be opened at the latter end of August the external pair of branchiæ will be found full of eggs in an early stage of development. Now may be seen the curious phenomenon of the rotation of the embryo within the chorion or membrane of the egg, first noticed by Carus. After the segmentation of the vitellus, the embryo begins to assume, in the space of about a month, the characteristic appearance which the young animal subsequently presents. The embryos remain in the pockets of the exterior branchiæ about eight

* *Lectures on Comparative Anatomy*, vol. i. p. 287.

† The orifices of the organ of Bojanus in the *Anodonta* are extremely difficult to detect. They are, however, readily seen in *Unio margaritifera*.

months—from September to April inclusively. And now comes a very curious part in their history: the young animals after exclusion are found to be parasitic upon fish, to the fins and gills of which they attach themselves by their barbed hooks. So different is the form of the embryo mussel from its parent that many authors have regarded these young ones as parasites in the adult animal; to these supposed parasites the name of Glochidia* was given by Rathke in 1797. How long these little creatures consider it necessary for their well-being to remain with closed valves upon the fin of an unfortunate fish, is a point which, I believe, has not yet been determined, and it is one, the solution of which I recommend to the investigations and patience of all *Intellectual observers*. Whether again this parasitism is absolutely essential to the growth of the young animal, or whether it can develope itself without such attachment, is a question which can only be answered by close observation and by experiment. I may refer the reader, who takes an interest in this matter, to my own paper "On the Fry of Anodonta Cygnea," in the *Quarterly Journal of Microscopic Science* (July, 1862), for the first record of the parasitic nature of these little molluscs.

There is a curious aquatic mite which is invariably found parasitic within the mantle and under the gills of the swan-mussel. These acari, which sometimes exist in great numbers within the shell, deposit their eggs within the folds of the inner branchiæ principally, though the ova and larvæ may also be seen attached to the interior surface of the exterior gills. Mr. Gwyn Jeffreys, who identifies this mite with the *Atax ypsilophora* of Buntz, says that it is "so tenacious of life that after the host has been put into boiling water and killed, these little parasites survive and crawl about as if nothing had happened!"

We must only just allude to the nervous system of the fresh-water mussel, which consists of at least three well-marked pairs of ganglions with connecting and branching nerves. (1) The labial ganglia are situated on the sides of the mouth; they are oblong and bilobed, and connected by a nervous filament. (2) The pedal or foot-ganglia are placed between the visceral mass and the foot; they are larger than the first-mentioned ganglia and more distinctly separated into two parts; this pair is connected by nerves with the labial ganglia. (3) The posterior ganglia are situated on the lower surface, a little below the posterior adductor muscle, near the anal orifice.

"All Lamellibranchs possess at least three pairs of prin-

* Gr. γλωχις, "a point."

cipal ganglia—a cerebral pair at the sides of the mouth, a pedal pair in the foot, and a third pair on the under surface of the posterior adductor muscle, which are commonly called 'branchial,' but which, as they supply not only branchial, but visceral and pallid filaments, may more properly be termed 'parieto-splanchnic.' Three sets of commissural filaments connect the cerebral ganglia with one another, with the pedal, and with the parieto-splanchnic ganglia. The inter-cerebral commissures surround the mouth, and the other two pairs of cords extend respectively from the cerebral to the pedal, and from the cerebral to the parieto-splanchnic ganglia."*

With regard to special organs of sense, the fresh-water mussels appear to possess only those of touch and hearing. The terminal portion of the foot is said to be most susceptible of touch, which sense may reside also in a slight degree in other parts of the body, such as the mantle and labial palpi. The organ of hearing, according to Siebold, consists of two round sacs containing fluid and an otolith each; they are situated on a pair of the nerves which spring from the pedal ganglia. The reader may readily observe the form and position of the auditory organs in the very common *Cyclas*; if the animal is removed from the shell and the foot be pressed between two pieces of glass and viewed under the microscope, the observer will see at its upper part two circular sacs with their contained otoliths lying on the nerves of the pedal ganglia; the otoliths keep constantly oscillating within the sacs.

"Aquatic molluscs, especially the acephalous kinds," M. Moquin-Tandon observes, "appear more sensitive of sound than the others (terrestrial). A very loud noise is necessary to cause a Helix to take refuge in its shell, or even to contract its tentacles. The *Limnæi* and the *Planorbes* appear a little less indifferent, but the *Cyclades* and *Unios* retract their syphon or their foot on the slightest disturbance of the air or water, even when one raises the voice or whistles near the vessel which contains them." "I have often remarked," says Baudon, "that the Anodons drew in their foot and confined themselves obstinately within their shell, when one spoke loudly or made any noise, and that without agitation of the water. One day when I had put *Anodonta cygnea* (var. *Celensis*) on a shelf in my room (they were out of water), I opened the door rather noisily and I saw my Anodons, whose feet were much elongated and were feeling the wood in every direction, I saw them, I say, draw in their feet at the sound of the door. I wished to repeat the experiment, and this time I opened the door softly with very little noise; then again the Anodons drew

* Huxley's *Elements of Comparative Anatomy*, p. 35.

quickly in their feet which a moment before was extended its full length; the same results have been obtained with the *Anodonta piscinale*, and very often I have had to draw in my breath while studying these animals."

I hope some of my readers will take care to prove for themselves the above-recorded fact. I confess I have not found these mollusca very susceptible of sound.

AIDS TO MICROSCOPIC INQUIRY.

II.—Capillary and Surface Actions.

Many of the phenomena presented by organized beings exemplify the physical actions of surfaces and capillary tubes. If two clean surfaces of substances that do not repel each other are pressed together, they adhere with more or less force. Plates of glass that are perfectly clean, exquisitely smooth, and accurately flat, may be made to stick together so tightly that they cannot be separated without breaking. In like manner plates of steel, if quite clean and worked with sufficient accuracy, will cohere, and only submit to separation when great force is applied. The explanation of these facts is that we have artificially reproduced the conditions under which the particles of solids naturally cohere. In order, for example, to form a block of iron it is necessary that the molecules of the metal should be brought very near each other, and that no substance should come between them capable of counteracting their mutual attraction. If, therefore, two surfaces such as we have mentioned fit so close to each other that the particles of one surface come within reach of the molecular attraction exerted by the particles of the other, they will behave as though they had formed part of a primitive undivided mass. If the contact is perfect at all points, we shall really have obtained a firm, solid mass; but it will often happen that the adhesion is incomplete at some portions of the two surfaces, and then the imperfect union will be broken through on the application of a force much less than would have sufficed to sever the parts of a block in a normal state.

If we take an ounce of shot and melt them in a crucible, we obtain a uniform mass of lead. The heat, by reducing the metal to a fluid state, enabled the particles to run together, or, in common language, to come into such close contact as to form one substance. If we take a piece of sheet lead, make its surface perfectly bright and clean, and then press it tightly against another piece similarly prepared, we shall have

obtained cohesion by another method. In the first case the heat, and in the second case the mechanical force of pressure, enabled the particles to come within the sphere of each other's molecular attraction, and thus to unite and form one body. The normal action of heat is to expand bodies, to increase the distance between their particles, and to give those particles greater freedom of motion than they had before.

The cohesion of liquids is far less than that of solids. It is easy to move an iron wire through a mass of melted lead; but when that lead is cooled and the mobility of its particles lessened, they refuse to get out of the way of the iron, and it can only be made to go through them by considerable force. When we have to deal with molecules or particles of the same kind, we know that they will cohere if brought near enough, and when the phrase "perfect contact" is employed, it does not mean that the particles really *touch* each other, but that they approach each other very closely, and thus imitate one of the conditions under which cohesion occurs.

It often happens that we desire to join two things together that we could not cause to cohere by the molecular attraction of their several particles. We then introduce a third body which has a strong tendency to adhere to both, and thus we glue or cement them to each other. In this case we have molecular cohesion between the particles of dissimilar substances; the film of gum, glue, or cement strongly attracts, and is strongly attracted by, the particles of the two bodies they stick together. Thus molecular attraction holds together particles of one kind, or particles of three kinds; if the glue was the means of joining two materials such as paper and glass together.

Physical attributes depend upon definite conditions. Glass is not *necessarily* hard and brittle, but is so necessarily under *certain conditions*. At one temperature it is like flint, at another it is like putty, and at another runs like water. Certain substances make sticky solutions; but we could not say that all substances which *can* make a sticky solution must do so whatever they are dissolved in. Copal makes a sticky varnish if dissolved in spirits and linseed oil, but in cajeput oil it forms a jelly not more sticky than that of calf's-foot.

The attraction which the surface of bodies exerts on other substances is most important. According to Magnus* all surfaces attract moisture from the air. Some surfaces do so with extraordinary power. To show this, put a little bit of chloride of calcium on a plate, and in a few minutes it will have taken enough moisture from the air to form a little pool. Sulphuric acid attracts moisture with considerable force. Thus

* See Intellectual Observer for June.

a vessel containing this acid will keep a glass case containing bright steel goods quite dry. If damp gets in, the acid seizes upon it, and the goods are protected from its effects. It is not safe to leave an open vessel of sulphuric acid in a damp cupboard for any length of time, for the moisture will condense and at length cause it to overflow.

It is true that in these, and many similar cases, we have something more than surface action, inasmuch as the whole mass of chloride of calcium, and the whole mass of sulphuric acid, becomes gradually affected by the moisture which the surface particles absorb, and then hand on in due proportion to their neighbours. The surface action is, however, not less real and important because other actions follow it in due succession.

A still more remarkable surface-action is manifested by the metal platinum. If a strip is *perfectly* clean, it will so strongly attract a mixture of oxygen and hydrogen gases as to make them unite and form water. The heat given out in the process of combination makes the metal red hot. Surface-action is proportionate to the quantity of surface engaged. Thus platinum in a minute state of division, constituting what is termed "platinum sponge," will instantly ignite a jet of hydrogen thrown upon it in common air; and many metals take fire in contact with air, if they have previously been reduced to so fine a powder as to cause an enormous and contiguous surface to be simultaneously exposed.

The lungs of animals are constructed so that large and contiguous surfaces of animal tissues shall be simultaneously exposed to the action of air. Gills do analogous work, and thus make the air that is found in water serve the purposes of life. Minute plants and infusorial animals do an immense quantity of work, because *in the aggregate* they bring a very extensive surface in contact with the fluids in which they grow.

If we wish to expose any substance to the most complete action some other substance can exert upon it, we endeavour to attack it at once on all sides. A lump of sugar would dissolve in the course of time if one point of it was kept in contact with water; but we dissolve it with far greater rapidity if we throw it into water, and let the fluid assail it all round and in every nook. This proposition is perfectly obvious; but the important consequences that flow from it are often overlooked. Suppose we take a flat piece of glass and bend it into a tube. If we put water in that tube it will be exposed to surface action all round. Suppose we reduce the diameter of the tube to the dimensions of a hair. It will hold less fluid, but every particle of what it does hold will be surrounded by a glass surface which will be close to it. Under such circum-

stances the surface-action becomes intensified. All porous bodies may be regarded as assemblages of capillary tubes, and hence they exert a greater effect. Charcoal, which is porous, absorbs gases with great power, and holds them so tight that they become condensed. Water and many other fluids rise in capillary glass tubes. Mercury is not attracted by, and does not attract, the glass in the same way. Pour water into a narrow tube, and the tube will seem to suck it up at the sides, producing a *concave* surface. Pour mercury into a similar tube, and the tube will seem to thrust it away, so that its surface will be *convex*, that is, highest in the middle when it is furthest from the glass.

When the surface of a capillary tube strongly attracts a given fluid, it cannot run through it easily; but if the repulsion of the fluid particles be increased, it will then flow freely. To show this hang a miniature pail containing water on the prime conductor of an electrical machine. Let a tube proceed from the bottom of the pail, so small in its bore that the water only drips slowly. Then turn the handle of the machine, and note, as the water becomes electrified, how rapidly it runs.

In passing through a narrow tube any fluid must experience *more friction in proportion to its bulk*, than the same fluid in larger quantity encounters in traversing a wider tube. Thus, on the ground of friction alone, it is much more difficult to force a fluid through a narrow tube than a wider one. If the tube and the fluid attract each other, the fluid will ascend as if by its own will. If they do not attract each other, much force will be needed to force the fluid on; and when circulation or other transmission of fluids occurs in the extremely minute vessels which the microscope reveals in thousands of organic bodies, we should consider the nature of the impelling force and the relation the fluid must bear to the sides of the little tubes through which it runs.

Some fluids will flow through tubes of a given size more readily than others, and if we pass from tubes to what we may regard as an assemblage of tubes—the pores of substances—we shall find that they exercise a physical choice as to what shall go through them and the rate at which the process shall take place.* "When two fluids are separated by a thin porous partition, either organic or inorganic, currents in opposite directions are set up between them, to which the names *endosmose* and *exosmose* are given. These terms, which signify impulse from within and impulse from without, were introduced by Mr. Dutrochet."†

* It is not proved that endosmose and capillary attraction are exhibitions of the same force; but many authorities regard them as such.

† Ganot's *Physics*, by Atkinson, p. 87.

If a bladder, filled with strong syrup, or other fluid denser than water, and capable of mixing with it, be immersed in water, and furnished with a tube that rises out of the water, it will be found that the water will enter the bag through its pores, and, by overfilling the bag, cause its contents to ascend in the tube.

For this sort of action to take place, the membrane or other porous body must be permeable to one liquid, if not to both; the liquids must be capable of mixing and differ in density. Gases can exhibit these actions; and when a fluid is capable of absorbing a gas, a membrane or other porous body may be the means of bringing a gas and a fluid into sufficiently close contact for chemical union to ensue.

It is evident that these considerations enable us to appreciate many particulars in the process of animal respiration, and in those changes in the contents of vegetable cells that result from the action and reaction that takes place between cell contents and the gases or liquids with which the cell walls come into contact and which they absorb.

The small organisms which the microscope is needed to reveal, are very frequently—we might say by far most frequently—of a soft and porous structure. Often they present no appearance of a distinct integument, and where an integument is present it is commonly readily permeable to air or water. Now, an organism with a permeable integument, or one composed of a permeable substance, must be subject to currents of endosmose or exosmose whenever its fluid contents differ in density from the fluid in which it is immersed, and the two fluids are capable of mixing together. Thus it will appear that animalcules or minute plants, such as desmids, confervæ, etc., will be continually subject to physical changes of this nature, and these physical changes will very frequently promote and bring about chemical changes, by which old materials will be re-arranged and new materials formed. We shall, in a future paper of this series, glance at such of the more obvious aids to the microscope which chemical considerations afford. At present we merely desire to call attention to the fact that physical agencies, under appropriate circumstances, provide the conditions under which chemical changes must take place.

We have treated surface action and capillary action as the same thing under different conditions. So soon as a set of particles belonging to a fluid, whether gaseous or liquid, come into contact with a surface, their liberty of action is curtailed. The particles of the solid surface attract or repel them, and the adjacent particles of their own nature which lie behind them are only capable of a partial resistance to the new force

that is introduced. The particles of water in a tumbler are all acted upon by gravity, and all have great freedom of motion from the highly fluid nature of the liquid of which each particle forms a part. The force of gravity pulls all the particles downwards with equal power, and, as the whole mass of water is liquid, the cohesion existing between the particles is not, as it is in the case of a solid, sufficient to sustain any one set of particles at a higher level than the rest. Consequently, unless a new force operating in another direction is introduced, a fluid surface will conform to the demands of gravitation, and be level—that is, equidistant at all points from the centre of the earth. At the sides of the tumbler a new force appears, and the glass, having an attraction for the water, raises certain particles above the rest. If any floating object be thrown in, it will either depress the fluid level in its vicinity by repelling the particles that are near it, or elevate the fluid level by the opposite action of hoisting certain particles up its own sides. In either case motion must be produced. You cannot pull up or down a certain layer of fluid particles without causing a wave, of a size proportioned to the amount of disturbance originally occasioned.

When a layer of particles is raised above a fluid level by the surface attraction of a partially-immersed body, the force which pulls the layer upwards is acted against first by gravity, and secondly by the attraction which the liquid particles have for each other; consequently, the amount of displacement actually produced depends upon how much greater the surface attraction is than the other two, and this again depends not only upon the actual intensity of the forces, but also upon the directions in which they act. If we take a rod of any substance easily *wetted*, that is to say, of a substance that has an attraction for the particles of water, we shall be able, if we first immerse such a rod in water and then withdraw it, to carry some of the water away. In so doing we have made the surface attraction of the rod overcome *gravity*, which was opposed to any elevation of the particles above the fluid level, and also the *cohesion* of the fluid, which was opposed to dissevering any of the particles from the general mass.

Endosmose and exosmose lead to currents in and out of porous bodies, and such currents may be made the means of moving bodies floating in a liquid. If we suppose the force arising from the currents to be equal in all directions, no motion will result; but, if the ingoing or outgoing currents are stronger at certain points of the surface of the floating body than at others, it will move more or less. It may oscillate if exposed to opposite and alternate impulses, or progress in one direction when the impulse is one-sided and continuous for a time. A

great ship drives a current of water behind it by means of a screw. As its own mass is large and heavy, it must move a proportionate quantity of water one way for the reaction to carry it in an opposite way. Paddles and screws hence make a noticeable turmoil, and a line of waves and foam marks out to the eye the wake of an advancing ship. When, however, we come to extremely minute bodies, measuring infinitesimal fractions of an inch, and weighing infinitesimal fractions of a grain, an amount of *action* upon the water, the corresponding reaction of which will cause the little body to move with extreme slowness through a space utterly invisible to the naked eye, will not give us even a miniature resemblance to the turmoil of paddle-wheels, and we must not expect to see, even with the microscope, that the little ship-like diatom will leave a water trail as it goes along.

Suppose an object in water moves one-thousandth of an inch in a second, and we magnify both it and the surrounding space one thousand times. When a second has passed, it will appear to have traversed a whole inch, and consequently its rate of progress will appear tolerably fast, if the thing itself even under that high power still looks small in proportion to the space through which it has gone. We should be wrong, however, if we expected to find the water disturbed; a very minute thing taking a whole second to get through about one-tenth of the thickness of an ordinary sheet of writing paper, would neither require a noticeable current to move it, nor cause a noticeable current when it moved. Endosmotic, or exosmotic, currents might move such bodies and be themselves unseen.

When fluids are in capillary tubes they are attracted (or repelled) on all sides by the tube surfaces, and no force can move them that does not prevail over the surface action, whatever it may be. A curious illustration of this occurs when fluids so situated are exposed to freezing temperatures. The water particles want to arrange themselves so as to form solid ice; but the capillary tubes hold them so tight that they cannot do so. "M. Despretz was able to lower the temperature of water contained in fine capillary tubes to —20° without their freezing."* The application of this fact to many microscopic organisms, which do not perish when the water containing them is frozen, is too obvious to need pointing out.

* Ganot's *Physics*, by Atkinson, p. 228.

STANDARD GOLD AND SILVER TRIAL PLATES.

BY JOSEPH NEWTON.

It has been quaintly but truly stated by an old and celebrated writer on the coinage of Britain, Ruding, that "the wisdom of our ancestors did not consider the private assay within the Mint as a sufficient security for the integrity of the coins issued from it, but required them to be submitted to a public trial by a jury before the Master could receive his discharge; and this trial was repeated at such short intervals, as to form a sufficient check upon improper issues of the money." It does not appear, however, that the ancients had any such trial, and the first record of one in this country is dated the 24th February, in the 32nd year of the reign of Henry III. (A.D. 1248), when the mayor and citizens of London were commanded to "choose twelve of the more discreet and lawful citizens" of the metropolis, with whom were to be associated "twelve skilful goldsmiths" of the same place. These twenty-four persons were to go before the Barons of the Exchequer, and having been sworn, were to examine, together with the Barons, collected samples of both the old and the new money of the realm, and to see that it was made of good silver and according to law, and so to conduct themselves that they might be able to warrant the same money as lawful or otherwise.

With more or less regularity, similar trials of the weight and fineness of the coin of the realm have taken place up to the year 1861, which is the date of the most recent examination of the gold and silver coins of England. The mode of effecting the trials has varied with the advancement of social and scientific knowledge; but in principle and object they have remained the same, and it is the practice at present to take one or more pieces from every 15 lbs. weight of gold, and one or more pieces from every 60 lbs. weight of silver coins struck at the Royal Mint, and to place them in what is called a "Pyx"* chest, in reserve for the general and public assay of their quality.

A tolerably explicit account exists in the Records of the Exchequer of a trial of the Pyx which was made in the reign of Queen Elizabeth, and from that document it may not perhaps be considered improper to introduce an extract, *verbatim et literatim*. It will be found to convey a very good idea of the ceremony, which, as has been stated, has not differed very materially from the days of that monarch to those of her present Majesty. It runs as follows:—

* The word *Pyx* is derived from a Greek word, signifying *box*.

"*Item.* The Co'nsell being assembled in the midle chamber, next the Mynt furnace in the Stare Chamber, the Tresurer and the other officers ought to bring in the saide pixe or pixes, locked with sev'all keys, before the saide Co'nsell, and then every bage, called Sinthia, for every moneth to be opened, and of so many pec's found in the said sinthia as wold make a pound w^ct^ of Silver or Gold, Troye, to be weighed by Troye wights, and after to be numbred, to try yf yt hold out in nomber, according to the Standerd and Indenture; and after that all the holle mony in every of the said baggs called Sinthia to be laid on one heape, and thereof taking so many pound w^tts^ as shall pleas the co'nsell to have put to the fyer to trye the Assaie. That done, the Wardens and Goldsmiths to be sworne to trye the said monye, and to take the Assaye accordyngly; the tenor of whose othe hereafter ensueth; that is to saye: Ye shall well and trewly, after your knolege and discrecions, make th'assais of theis monys of Gold and Silver, and trewly report yf the said monys be in wight and fines according to theis the King's Standerds of his Treasury,* and allso yf the same monys be sufficient in allaie, etc., according to the covenant comprised in the Indentur thereof mad betwyn the King's grace and the M^r^ of his Mints, so help y^e^ Godde.

"And that done, one of the said severall pond w^tts^ of mony put in severall fier potts to be delivered to the Foremane of the Jurie, to be molten and tryed by the Assaye, wherby yt may be knowen whether the said pound w^tts^ containe so many oz. and peney w^etts^ in puer and good Syllver as by the Standard and Indenture is apoynted or not; and, as then yt shall be found by the said Jurye, to be syngnified unto the Co'nsell, whos verdit with the nomber of all the other coyned by any suche Indenturs, within the tyme of the said Assaye, to be delivered from the said Co'nsell to the Remembrancer to record all ther doings accordingly as apertayneth.

"*Item.* The lyke assaye to be mad of the Gold, mutat'mutand."

No doubt the form of making the Trial of the Pyx as here prescribed was derived from an earlier period, as the "King's" standard is mentioned and not the Queen's. It will be apparent that, in order to make the examination as to the purity of the pyx coins perfect, it was necessary that some standard or infallible tests should exist to which comparisons might be made. These were called Trial Plates, and they consisted of portions of the precious metals, the composition of which had been studiously effected in accordance with the legal enactments relating to the fineness of gold and silver coins.

* The Trial Plates, to be presently further referred to.

At the Mint there exists, said the late Mr. Bingley, Queen's Assayer (who took great pains to ascertain the fact), no authentic information as to the actual existence of standard plates anterior to the year 1660; but it is morally certain that their formation must have been coeval with the institution of the pyx trials. In a previous paper published in this journal,* it was stated that the quality of the British coinage had fluctuated under various monarchs to a slight extent, but that for several centuries it had remained unaltered. This latter fact is due, no doubt, to the increased skill and improved workmanship which, in modern times, has distinguished the *employés* of the Mint, as well as to the higher standard of morality which has obtained among our rulers. It is clear, nevertheless, that trial plates have not been without *their* influence on the purity of the coin of the realm. They have afforded the readiest means of checking the quality of the metallic currency which could be devised. It is, beyond doubt, possible for skill and integrity to keep coins up to any particular quality of fineness; but as this involves also personal responsibility, it is assuredly necessary to have, as an arbiter of disputes, and as a guarantee to the public of the honesty of its servants, standards of excellence, religiously kept, but to which periodical reference may be readily made. That this feeling has been always entertained is proved by the circumstance that Government has always been provided with trial or test-plates, by comparison with which the fineness of the coin might at once be estimated. It was also deemed advisable, as long back as the year 1660, that the manufacture of the plates should have been trusted to those who, not being in any way connected with the Mint, could have no possible interest in deviations from strict accuracy of workmanship. The Company of Goldsmiths appeared to be the fittest persons for the task; and from the year named to this hour, they have accordingly been empowered to accomplish it. It is unnecessary to say that they have in all cases acquitted themselves to the satisfaction of the Government, and to the best interests of the public.

It may not be improper here to state that, having once determined on the quality or fineness of the intended trial-plates, there are yet several difficulties to be surmounted in their formation. Should there be any error made in the quantity of either the fine metal or the alloy to be combined with it, the resulting plates would be imperfect. Again, should either of the two metals to be united be impure, even in the slightest degree, the necessary homogeneity would not be attained from their mixture in the crucible. Allowing that the weight and

* *Vide* No. 14.

the purity of the ingredients are perfectly certain, still it is possible that in their combination errors may creep in. If, for example, the mass be kept too long in a state of fusion an oxydation of a portion of the alloy will take place, and thus render the casting too fine; if there be not sufficient heat in the furnace, or if the melted mass be not duly stirred so as to effect a proper chemical action and consequent union of the two metals, the plate will assuredly vary in purity in different parts. There are other practical points to be attended to in the creation of new trial-plates, but, of course, in the hands of the Goldsmiths' Company they are not at all likely to be overlooked. It is not often, indeed, that trial-plates have to be renewed. A small portion only of each, whether of gold or of silver, is consumed at each Pyx inquiry, and they last therefore for many years. In 1660, a gold trial-plate was made by the Goldsmiths' Company, this was followed by another in 1668, the next was cast in 1707, a fourth was produced in 1728, and that which is used at present came into existence in 1829. Silver plates were made on the same occasions, and it does not appear that at any time a new gold plate was made without a corresponding plate of silver being also prepared, and *vice versâ*.

The gold trial-plate now in use as a check upon the British Mint may justly be pronounced of the true standard. It has been certified to be so, indeed, not only by the Goldsmiths' Company, but by the authorities of the Mints of Paris and of Amsterdam, to whom small pieces of it were sent for examination. The same remark will apply to the silver trial-plate. The forms of both are nearly identical. They are simply wide strips or ribands of the respective metals, with corrugated indentations running transversely across their surfaces, and bearing inscriptions as follows:—

Goldsmiths' Hall, London.
This Standard, commired of eleben ounces two pennyweights of Fine Silver, and eighteen pennyweights of Alloy in the pound weight Troy of Great Britain, was made the 31st day of October, 1829.

Goldsmiths' Hall, London.
This Standard, commired of twenty-two carats of Fine Gold and two carats of Alloy in the pound weight Troy of Great Britain, was made the 31st day of October, 1829.

They also bear impressions from the half-sovereign die which was in use at the Mint on the date mentioned, and are accompanied by certificates, signed by the then Master of that establishment, the Hon. J. C. Herries, and the two Assay Masters, Messrs. Bingley, and Beckwith, guaranteeing their exact conformity to standard.

The actual trial of a Pyx, omitting the form and ceremony observed, and which we may take a future opportunity of referring to, consists in subjecting minute portions of the plate,

whether of gold or silver, and of the coin, to the same process, and, at the same time, for separating the alloy from the precious metal. The result shows whether the coin subjected to the assay is more or less fine than the plate, and in what degree.

It will be obvious that in the employment of this method the accuracy of the plate is of the utmost importance, because the trial does not determine immediately the relative fineness of the coin assayed to the standard of the realm, but only to the trial-pieces, which are presumed to be exactly conformable to that standard. Before taking leave of this subject, it may be well to define precisely what are the standards of the gold and silver coins of England, both as regards fineness and weight, together with the remedy allowed at the Pyx.

1. The standard in fineness of the gold and silver coins of the realm is as follows:—

Gold—Twenty-two carats in one pound troy weight (or eleven twelfth parts) of fine gold, and two carats in one pound (or one-twelfth part) alloy.

Silver—Eleven ounces and two pennyweights in one pound troy weight (or thirty-seven fortieth parts) of fine silver, and eighteen pennyweights (or three-fortieth parts) alloy.

2. The standard in weight of the gold and silver coins of the realm is as follows:—

Gold—Nine hundred and thirty-four pieces and one half piece of the denomination of a sovereign to weigh twenty pounds troy weight, being at the rate of nearly one hundred and twenty-three grains and two hundred and seventy-four one-thousandth parts of a grain to each sovereign, or piece of the value of one pound sterling, and so in proportion for pieces of greater or less sterling value.

Silver—Sixty-six pieces of the denomination of a shilling to weigh one pound troy weight, being at the rate of nearly eighty-seven grains and two hundred and seventy-two one-thousandth parts of a grain to each shilling, or piece of the value of one-twentieth part of a pound sterling, and so in proportion for pieces of greater or less sterling value.

REMEDY.

3. The remedy, or allowance, made at the trial of the Pyx for fallibility of workmanship, is as follows:—

Gold—One-sixteenth part of a carat, equal to fifteen troy grains in fineness, and twelve troy grains in weight, in one pound troy weight of gold coin, whether varying above or below the standards of fineness and of weight respectively; being, as regards weight, at the rate of two hundred and fifty-seven one-thousandth parts of a grain on each sovereign or

piece of the value of one pound sterling, and so in proportion on pieces of greater or less sterling value.

Silver—One pennyweight or one-twentieth part of an ounce in one pound troy weight of silver coin, whether varying above or below the standard either of weight or of fineness.

It will thus be seen that the Mint is legally restricted to a very close approximation to the true standards, of which the trial-plates are the exact representatives. In consequence of this all coinages of the precious metals executed at that establishment have to be conducted with excessive care as regards the mixture of the alloys with those metals, in order to avoid the errors and evils which might otherwise be fallen into, and to ensure to the public that no debasement of the currency result either from negligence or malversation. Not a single bag or journey-weight of 15 lbs. of gold coin, nor a single bag or journey-weight of 60 lbs. of silver coin is permitted to pass from the Mint presses to the Bank of England without at least one piece, taken at random from it, being impounded and placed under seal for testing against the trial-plates at some future day. The nomination of that day rests with the Lords Commissioners of Her Majesty's Treasury, and the trial itself is presided over by the highest legal officer of the State, the Lord Chancellor.

The advocates of the metric system of weights, measures, and money, are desirous of lowering the standards of fineness of gold and silver coins of the United Kingdom, and of assimilating them, in fact, to the standards of France and of the United States of America. If this proposition were acceded to, the proportion of fine gold in our sovereigns and half-sovereigns would be nine-tenths, and that of copper, one-tenth, whilst our silver monies of every denomination would contain nine-tenths of fine silver and one-tenth of copper in lieu of the existing standards. We offer no opinion upon this point, except to say that it might be found inconvenient to have coins in simultaneous circulation of two descriptions of fineness, and that it is probable that the colour of the gold, if not that of the silver pieces also, would not be improved by the larger admixture of copper. It is to some extent a chemical question as well as a social one, and no doubt it will be carefully considered in all its bearings before being answered affirmatively.

ON COLOURED VISION PRODUCED BY SANTONINE.

BY DR. PHIPSON, F.C.S., LONDON, ETC.

In my work, *Phosphorescence; or, the Emission of Light by Minerals, Plants, and Animals*, I have described (pp. 162—164) a series of rather curious phenomena, under the designation of *Subjective Phosphorescence*. They occur whenever the optic nerve receives the slightest shock, or an injury of any kind; they present themselves in certain diseases, such as typhus fever, measles, etc.; they are determined by sudden increase of circulation, by the action of a weak electric current through the eye, by the use of certain narcotic medicines, etc. In these cases we see a more or less vivid production of *light*, which is all the more interesting as it occurs *within ourselves*, establishing the fact that light (in these circumstances certainly) is produced by a *vibratory motion of matter*. It shows itself either in form of revolving specks, radiated lines, or the arborescent figures described by Purkinje, etc. In the present paper I have to treat of another set of physiological phenomena quite as interesting as those just referred to, and bearing a certain relation to them. I allude here to the production of *coloured vision* by means of a substance called Santonine, a crystalline principle which exists in the plants of the wormwood tribe (*Artemisia*), and in the *semen contra* of pharmaceutists.

"Semen contra vermes," or simply "semen contra," is the name given to the mixed flowers and seeds of *Artemisia judaica*, L., a plant native of Arabia and North Africa. In commerce there are various kinds of semen contra. The French and Belgians call it sometimes Sementine or Barbotine. *Artemisia contra* is another plant which furnishes it. These plants are met with also in Tartary and Persia, and are cultivated in our gardens. The semina santonici, or santonicum of pharmacy, is a powdery substance, with a strong aromatic odour and very bitter taste, resulting from the breaking up or coarse pulverization of the flower-heads and stalks adjoining; the flowers being partly gone to seed, the whole is a mixture of seed, flower stalk, and leaflets. This product is taken internally to kill intestinal worms, at a dose of about one drachm in powder alone, with honey or otherwise, repeated two or three times a day; and when it has been continued for some days, a strong purgative is administered, which completes the cure.

Artemisia absinthium, another plant of the same family, and, in general, all the Wormwood tribe, possess, more or less, the same medical properties. In England we have a number of *Artemisia*, known as wormwood, southernwood, and mug-

worts. *Artemisia campestris* is somewhat rare, but is to be met with on dry, sandy heaths in Norfolk and Suffolk, where it flowers in August. It is called field southernwood.

Artemisia absinthium, or common wormwood, is found more or less abundantly in waste places and around villages, in dry soils; it flowers also in August, and stands about one foot and a half high. *Artemisia maritima* is a small grey shrub, seen growing on the sand downs near the sea-shore, and met with also near salt springs and salt marshes in the interior. Like all the plants of this genus, it gives out a peculiarly strong aromatic odour when the leaves are violently rubbed in the hands, and has an intensely bitter taste.

Artemisia vulgaris (mugwort) is the commonest of our species of *Artemisia*. It grows three or four feet high, and is found along hedges and in waste places. Like the others, it flowers in August. These plants belong to the great family of *Compositæ*, and to a group which is characterised by many bitter plants highly extolled by the old herbalists and alchymistic medicine-vendors of times gone by. Their leaves are much divided (pinnatifid), more or less downy, and the flowers yellow.

It is not surprising that a substance like semen contra, so extensively used in medicine for many years, should have been carefully examined in a chemical point of view. All the plants we have just named contain the peculiar principle called Santonine; we shall see presently how it is obtained. Under the name of Barbotine is known on the Continent the mixture of buds, blossoms, seeds, and flower-stalks of various *Artemisia*, dried and reduced to a coarse powder. It is the same as our semen contra. We possess an elaborate analysis of it by Tromsdorff, and two analyses quite as elaborate by Wackenroder. There is, however, little interest in reporting here the figures of these analyses, made many years ago, and in which certain figures include more than one substance. It will be sufficient to state, that the medicinal properties of the wormwood blossom are owing partly to a volatile oil, similar in nature to essence of peppermint, partly to the bitter resins it contains, and to the crystalline principle Santonine.

Santonine was discovered about the same time by two German chemists, Kahler and Alms. They extracted it from wormwood by treating the flowers, buds, and seeds (semen contra) of the *Artemisia* with ether, which dissolved the Santonine; or by treating the flower-heads, first with alcohol, evaporating to get the alcoholic extract, and treating this with ether. On distilling the latter, the Santonine remains behind, mixed with some chlorophylle or green colouring matter, and a little resin or wax. This mixture was dissolved in strong acetic acid, which takes

up the Santonine and deposits it in crystals when the liquid is allowed to evaporate in the open air. Some time afterwards, Tromsdorff junior submitted Santonine to a very careful and complete study, and gave a new method for preparing it. This method is now generally adopted. It consists in mixing 4 parts by weight of the dry semen contra with 1½ parts of caustic lime, and extracting the mixture three successive times with 16 to 20 parts of strong alcohol. The liquid thus obtained is distilled until reduced to 12 or 16 parts. It is then filtered to separate some flocconous matters which are deposited. The filtered liquid now contains only Santonine and lime; for Santonine acts as an acid, and combines easily with soda, potash, lime, etc. It is evaporated to half its volume, mixed while hot with an excess of acetic acid, and allowed to cool, when Santonine crystallizes abundantly. It may be carefully washed with a little alcohol, and then crystallized again. By dissolving it in alcohol, bleaching the liquid by adding a little animal charcoal, and filtering, perfectly pure Santonine is obtained, in the shape of flat, six-sided prisms, quite colourless and odourless.

As Santonine is insoluble in water, or nearly so, it has no taste unless it be masticated for some time, when a slight bitterness is perceived. Its solution in alcohol has, however, a very bitter taste. It is fusible, and can be sublimed to a certain extent; but if the temperature be rather high, it inflames and burns with a bright smoky flame. Its best solvent is alcohol; ether and essential oils dissolve it likewise, and fixed oils also take up a little; but it is very slightly soluble in water. Its specific gravity is 1·247.

As regards light, Santonine possesses peculiar properties; it refracts light very strongly, like the diamond or like quartz; and when exposed for a few minutes only to the direct light of the sun, the white crystals turn to a brilliant yellow, which becomes rather darker if the action of the sun's rays be continued for any length of time. As this change only occurs upon the surface of the crystals, and forms a very thin layer of yellow matter, which protects the inside of each crystal from any further change, the composition of the substance appears to remain the same; but if the action of light were caused to continue until the whole mass of the crystals was changed into this yellow substance, I have no doubt the latter would be found to differ in more than one respect from the original Santonine.

Santonine is prescribed, more particularly by German physicians, as a vermifuge for children; but we have many reasons for supposing that its action in this respect is less efficacious than that of the plant, the semen contra, or Barbotine itself.

The most curious property of Santonine is certainly its physiological effect upon the vision. Experiments were made upon

this peculiar action, almost at the same time, by myself and by M. de Martini, of Naples. I shall proceed to detail the more remarkable of these experiments.

At a quarter past two o'clock, on the 5th February, 1859, I took five grains of Santonine, and waited anxiously for the desired effect, namely, the production of coloured vision. For a considerable time I experienced nothing extraordinary, and began to think the experiment a failure. For, as I never have taken medicine of any kind, except now and then as an experiment, my constitution is particularly sensitive to the influence of any chemical agent. About five o'clock, however, I fancied that the white curtains of my room had a very *pale greenish tint*. I attributed the effect to imagination, and went out. At six o'clock I returned, the gas was lighted, and the fire was burning brilliantly on the hearth: a singular phenomenon now presented itself. The fire and the glass globes which surrounded the gas-jets appeared of the most gorgeous yellow it is possible to imagine. It was a *greenish-yellow*, exceedingly rich and brilliant, resembling some kinds of burnished gold. It was the colour of chlorine gas, but much more intense. I communicated these effects to the persons near me, who, seeing the objects as usual in their natural colours, could not realize what I experienced. Everything in the room that was white and strongly lighted (as the tablecloth for instance) appeared to me of the most brilliant greenish-yellow, the flames of the gas-jets and the fire were most remarkable in this respect. The other objects of different colours, red, blue, yellow, appeared still red, blue, or yellow; it was only white objects and flames that appeared of that peculiar greenish-yellow tint alluded to; but, doubtless, *all* tints must have been more or less affected by the state of my vision.

It is, perhaps, needless to add that I experienced no disagreeable effects, at least not at the time I experienced coloured vision, though some time before I felt slightly bilious. As the colour seen was, as above stated, nearly that of chlorine gas, I prepared a bottle of this gas and held it to the light; it appeared of its ordinary greenish-yellow tint, but much more intense than usual. The flame of a gas-jet appeared, in these experiments, of a bright *greenish-orange*, very rich, and the lower portion of the flame, where a vivid rim of blue light is observed in ordinary circumstances, appeared to me of a beautiful bluish-green.

These effects continued *without intermittence* during the rest of the evening; they began to diminish about half-past ten, but were very distinct, though weak, at twelve o'clock, and even until two o'clock at night, when I retired to

rest. The next day all effect had disappeared. The Santonine employed in these experiments was extracted from *Artemisia santonica*, or *judaica*, and was absolutely pure.

M. de Martini, of Naples, made several experiments upon his patients and his pupils, in order to ascertain whether the colouration of vision, by Santonine, was the same for every person, and whether the dose had any influence upon the colour seen. The results obtained are certainly remarkable. An invalid, who was taking Santonine as a vermifuge, invariably noticed about twenty minutes * after the prescribed dose had been administered, *all* external objects of an *intense green* colour; whilst a student, M. Cassano, constantly saw objects of a *blue* colour. But in the great majority of cases it was observed that persons who had taken Santonine saw external objects of a *straw-yellow* or *greenish-yellow* tint. The dose appears to have a very decided influence upon the results. A young man who took five grains of Santonine in the solid form, saw external objects of a *yellow* colour; the dose was then doubled, and thirty-six minutes afterwards, objects no longer appeared green or yellow, but perfectly *red*; half an hour later they appeared *bright orange*, and later still, *yellow* as before. With M. Cassano, whether he took five grains or ten grains, external objects always appeared to him of a bright *blue* colour, whilst his fellow-student, M. Pedretti, saw objects always *straw-yellow*.

Another curious fact was observed, which I have looked for in vain when experimenting upon myself, namely, an *intermittence* in the effects of coloured vision. In some individuals the apparent colouration of external objects is not permanent, but intermittent; it disappears for five or six minutes, and then returns as strong as before. This I have not remarked; the greenish-yellow tint observed on all white objects and flames was constant from the beginning to the end of the experiments, it became gradually less intense only as the effects of the Santonine went off. In no case has this remarkable action of Santonine upon vision been seen to last more than a day. The experiment may be safely made, even by delicate persons, with a dose of five or six grains; I do not think it would be reasonable in any case to experiment on more than ten grains at a time.

The cause of this peculiar action of Santonine is the next thing to be examined. M. de Martini believes that this substance exercises a molecular action upon the retina or optic nerve; others, among whom are M. Miahle, of Paris, think

* Twenty minutes is a very short period, unless the Santonine had been taken in alcoholic solution; I am informed that, if taken as an enema, the action is remarkably rapid, and coloured vision produced in about a quarter of an hour.

that, on being introduced into the system, it induces a momentary bilious condition, which passes off in the course of the day. I also imagined, in my first experiment with Santonine, that it induced a slight biliousness, but I neglected to observe this in other experiments. In jaundice, as is well known, the serum of the blood takes a yellow colour, and the urine becomes so yellow that it easily dyes a white cloth dipped into it. Yet I have not heard it remarked that patients suffering from jaundice, have ever seen objects otherwise than in their natural colours. Leroy d'Etioles, lately one of the most eminent medical practitioners in Paris, observed some time ago, that Santonine, taken by children as a vermifuge, communicates to their urine a *greenish-yellow* colour. I have also observed that Santonine, or some substance derived from it, passes into the urine of those who take it, and M. Miahle has made a similar observation.* Now, no substance can find its way into the urine without passing through the blood. The presence of Santonine, or any compound of Santonine, in the urine, is, therefore, a proof direct that, in spite of its slight solubility in water, Santonine is taken up, in some shape or other, by the blood, and circulates through the system. The alkalinity of the blood may facilitate its absorption, and it may be taken up as santonate of soda, a tolerably soluble salt. But it is far more probable that Santonine is transformed into a new substance, of an intensely greenish-yellow colour, to which I gave the name *Santoneine*, and obtained by boiling Santonine with nitric acid. I believe this is the substance that passes into the urine, and which forms on the crystals of Santonine by the action of light. However, the chemistry of this product is still very incomplete. Admitting that it gets into the blood, as the greenish-yellow substance Santoneine, it is no longer so difficult to account for the production of coloured vision. The retina, or expansion of the optic nerve, is a translucid membrane which receives numerous branches of the ophthalmic artery, and sends a branch to the transparent vitreous humour of the eye; so that the ingestion of Santonine colouring the serum of the blood, the latter is capable of colouring the transparent media of the eye sufficiently to produce the effects described.

In the present state of our knowledge of the chemistry and physiology of Santonine, this appears to me the only manner in which we can at all account for the production of coloured vision, for Santonine does not appear to have any action whatever upon the nerves.

* The urine of persons who have taken Santonine, evaporated to dryness, and the residue treated with boiling alcohol, yields a solution which turns reddish-orange when potassa is dropped into it, a character which serves to indicate Santonine.

If any of my readers should desire to repeat these experiments on coloured vision, it is essential that the Santonine employed should be obtained from reliable sources, for I see in the papers that an apothecary has recently met with a sample of Santonine containing *strychnine* (!)—the result, doubtless, of an accidental mixture of the two substances, by a careless assistant or otherwise, and, we hope, not likely to occur again; nevertheless, I think, the fact should be recorded here. Pure Santonine has no taste when a crystal is placed upon the tongue.

INSTINCTS AND HABITS OF BEES.

BY SHIRLEY HIBBERD.

Among the many out-door pursuits that attract enthusiasts and furnish subjects for meditation to the thoughtful, there is nothing to be compared with bee-keeping. If a man has but one stock of bees and is of the right temper to make pets of them, his attachment to them will grow so surely that it will be strange if he does not, in a very short time, renounce many a commonplace pleasure in order to make room in his heart for a strong affection for these happy confectioners, and perhaps appropriate a portion of his head to an investigation of their instincts and habits, so as to prove for himself all the written records of bee history, and live in hope of adding to them the results of personal observation. Spite of the immense extent of bee lore, if we only go so far back as the fourth Georgic of Virgil and close with Huber, there are quite as many problems yet remaining unsolved as have been solved already, and those who lament that the fields of discovery are exhausted, only need set up a bee-hive and put Huber to the proof throughout, and it will be found that though we know much about bees we do not yet know all. I often sit beside my hives and speculate on the mysteries of instinct and its curious blendings with intelligence, and during the glorious weather of April and May of the present year, the bees have given new life to all the old facts, and made me feel that one fact gained by observation is worth a thousand drawn from books; and in the matter of bees seeing is believing, and it is scarcely possible to believe by any other method of proof or persuasion. The nightingales have been singing at Stoke Newington this season as gaily as ever, and the bees are thriving amazingly, yet all around us the builders are drawing a close cordon of bricks, and it is fast becoming a problem how bees are to find food here, for the hedgerows are

entirely gone, the groves of lime are thinned and their sites marked out for houses, and in place of clover fields and florists' grounds, rich in stores of honey, there are myriads of villas rising on every hand, and, with me at least, bee-keeping is becoming a purely metropolitan undertaking. Yet I see no falling off in the industry of my busy friends, no diminution of the harvest, no deterioration of the quality of the honey. The box of snow-white virgin honey which I exhibited five years ago at a meeting of the Apiarian Society in Bedford Row, can be matched with combs now in use from last year's harvest, and as I sit beside the hives, the bees come in laden with golden and russet pollen as bright and plentiful as of yore, but where and how they obtain their supplies is a mystery which defies explanation. I suppose, in order to look wise about the matter, it must be said that bees can travel three or four miles, and that the open country still lies before them with its wealth of honied spoils; and so long as London does not cover the whole county of Middlesex, the bees will find their pasture, and treat with proper contempt the invasion of the meadows by the speculative builders. But the growth of the town does make its mark in the annals of the hive. When I first took up my abode at Stoke Newington, we used to shut out London by slamming the front gate; all beyond was a region of meadows and gardens, rising in successive swells to Muswell Hill, which bounded our prospect from the topmost windows. Now the view is intercepted on every hand, the enormous mass of houses known as Highbury New Park, the newer village known as Woodbury Down, and last and worst of all, the rurality of Lordship Road is extinguished, and new bricks glare upon the eye where mighty elms and pines waved their green banners against the blue sky, and furnished villages for mellifluous nightingales and clamorous rooks. I have read all this in the character of the honey produced here, for under certain conditions the produce is dark-coloured, and when the honey is run off from the comb there is deposited by it a black sediment which is real soot; yes, vulgar soot. When I say that last year's honey was as white as the sample which elicited such unanimous and enthusiastic admiration five years ago, I must add that such honey is only to be obtained by preventing swarming and putting on the honey-boxes at the latter end of May. Then the boxes are *filled quickly* at a time when few fires are burning, and the result is a product equal to the best obtainable in good bee districts in the country. If they are capped early, and the caps, through the stocks being weak or the weather being cold, are a long time filling, the comb is dark, and we are obliged to confess that it is London honey, and, like all London productions, sooty.

I must confess that, though my spouse keeps a sharp eye on the economical results of bee-keeping, I am careless of that matter, because much more interested in the mysteries of that perfect monarchical system which a bee-hive represents, in its estates and orders, its community of interests, and divisions of authority and power. Perhaps there is also some gratification of vanity in being able to handle bees dexterously, and indulge in a species of familiarity with them which is denied to mortals who have no apiarian proclivities. If I can work the whole day through in planting, building, and superintending gardening operations within a yard of the mouths of the hives, while the population are rushing to and fro, and making a buzz somewhat resembling the roar of the sea on the beach at half a furlong distance, there must be a friendly familiarity between us; or how is it that I alone can do so with impunity, having no fear of those awful weapons for which bees are deservedly notorious? But I shall not boast of this familiarity, for in plain truth I disbelieve it, though daily experiencing a *quasi* proof of the claims of the apiarian saw, that bees know and love their keepers. I am not only busy myself at the mouth of the hive at midday in hot weather, when no one else dare come near, but I can take up a bee just returned from a journey, carry it away in my hand to a distant part of the garden, and then set it free, in order to see it first dart upwards and slightly circle round, and then go in a straight line, like a shot from a gun, to the mouth of the hive again, which is a homely illustration of the American method of hunting for honey in the prairies. With all this, and much more that would be trifling to relate, I do not believe the bees know their keeper, or have the slightest care or affection for any human being, Virgil, Columella, and Wildman not excepted. To be used to bees is one thing in favour of these operations. A nervous person will be sure to make a fuss, and perhaps shake the hive-board in attempting to operate with bees; but experience renders one capable of moving easily and quietly and with self-possession, and what we call "nerve" in such matters is as needful in taming lions as bees, and is the first essential of success. Now why should the bees know me? I have not seen much of them all the winter, and the bees I played and talked with last summer died long ago, and those now buzzing in and out have scarcely seen me till lately; and there cannot be much friendship, for the simple reason that there is, *ab initio*, little or no acquaintanceship. The fact of the matter is, that bees are very loath to sting anybody when they are thoroughly busy; and so long as their causeway is not interrupted, they do not for a long time become aware of the presence of an intruder on their precincts. The bee-keeper's first care is to keep the

causeway open; this done, he may approach the hive at the side of the entrance, and do almost as he pleases short of inflicting actual injury. By the word "causeway" is not meant the entrance to the hive; that, of course, is always open; but the space through which the bees pass in their way from the hive to their pasture, and from their pasture to the hive, and which on a hot day is easily distinguished by the bees themselves, who perform their traffic, as it were, in a channel, or shall we say a "sailing line," from which they only diverge when they are at some distance from the hive. Let us sit beside the hive and watch. One after another the bees swarm out; each bee rushes to the edge of the hive-board, and from that edge darts forward along the sailing line, then makes an easy curve upwards into the free atmosphere and is gone. Stand for a few moments in that sailing line, and you will have warning: a bee, interrupted in her progress by your presence, will buzz about you, and unless you move at that hint, one or two more will join in the quarrel; next, war will be proclaimed in a loud key, and unless you retreat quickly and far away, you will taste the venom of a bee's sting, and remember for the rest of your life the potent chemistry with which Nature has endowed this little creature for self-defence. But you see the habit of the bee is to prefer industry to warfare, and it rarely happens that a person is stung by a bee without having fairly earned the compliment by blocking up the causeway, or in some other manner interrupting the sober and serious business of the hive. The requisites for bee-keeping are not so much skill as care—not so much a knowledge of bees as confidence in handling them. With these two requisites a little knowledge goes a great way, and all the necessary practical details of bee-keeping may be learned in a season.

Any one given to habits of observation will, in taking notes of the operations of the hive, soon be led into speculations as to the relative degrees of the sensational functions in the bee. That they are not greatly dependant on eyesight is evident in the fact that they prefer to work in the dark. Yet that they are very sensitive to the impression of light is evident by their eager rush to the open air when the ground is covered with snow; for then, and only then, it becomes necessary to stop the entrance for a time, or the bees, probably mistaking the glare of light for sunshine, sally forth gaily, soon get benumbed and myriads of them perish, so that strong stocks are often seriously weakened by snow-storms, unless kept at home by the watchful apiarian. That they are sensible of vibrations may be proved any day by tapping on the floor-board with the knuckle, for there is at once a rush to the entrance, and a dozen or more workers, with a real look of wonderment and

curiosity in their aspect as they explore the alighting-place, seek the cause of the disturbance, and send out a few scouts to circle round and see all safe, or, it may be, signify by threats of stinging the disturber of their peace, that it will not be safe to repeat that experiment too often. There is no doubt that the transmission of signals and communications necessary to the progress of the work within the hive is as much accomplished by the beating of wings and various humming tones as by the contact of antennæ, for many distinct sounds of bees can be distinguished by observation as associated with distinct acts, *e. g.*, the sharp ringing sound that immediately precedes the act of stinging: who that has heard it, and felt the result, can ever forget it as long as he lives? When a bee is hurt, as, for instance, in putting on a honey-box the edge of it will sometimes fall upon some unlucky bee, the same sound is produced, expressive, doubtless, of anger more than of pain.

Shakspeare, who drew comparisons and metaphors from the meanest things, makes use of this habit of the bee in that magnificent scene in "Julius Cæsar" (Act v. s. 1), where the conspirators encounter Octavius and Antony:—

> CASSIUS. Antony,
> The posture of your blows are yet unknown;
> But for your words, they rob the Hybla bees,
> And leave them honeyless.
> ANTONY. Not stingless too.
> BRUTUS. O yes, and soundless too;
> For you have stolen their buzzing, Antony,
> And very wisely *threat before you sting.*

That the sense of smell is very acute in bees does not admit of question. No doubt the immunity many bee-keepers enjoy from their attacks, in their bold methods of manipulating at the hive, is in some measure attributable to the fact that the emanations from the skin are agreeable to the bees, or at least not offensive. On the other hand, the fatality that attends the interference of others, may be attributable to the emission from their bodies of odours which bees dislike, as well as to clumsiness and want of courage. I had in my employ for some years a gardener who could never go near the hives at any time, except in wet or cold weather, and not always then with perfect safety. I have seen him at work at a distance of a dozen yards from the bee-shed, thinking himself perfectly safe, but presently a bee would lodge in his neck, inflict a wound before he could move a step or put up his hand in defence, and leave him writhing with pain, and, of course, with perhaps a spice of sarcasm, pronouncing it very marvellous that at the same time I could go as near the hive as I pleased and quietly liberate a bee from a spider's web and place him on the board, to be cleaned of

the defilement by half a dozen members of the family, all visibly solicitous for its welfare and safety. Wildman was one of the most expert of manipulators ever known; he could do anything with bees, until he had an attack of fever, and forthwith his apiarian necromancy left him; he could no longer operate upon the hive with safety; the bees smelt him out and punished him, as he said, because the emanations from his skin were no longer pleasant to them. Mr. James, in his charming essay on the "Honey Bee," conjectures that the fever had robbed Wildman of his self-possession; but it is worth a second thought whether the explanation of the great apiarian was not, after all, correct; his physical system had undergone a change, and, like the quails to the Israelites, he now "stunk in their nostrils."

In the *Pleasures of Memory* (Part I.), Rogers imagines the bee to find her way back to the hive by retracing the various odours of the flowers she passed successively in her journey outwards:—

> "With conscious truth retrace the mazy clue
> Of varied scents, that charmed her as she flew,"

which is neither more nor less than a poet's fancy, as any one may prove by simple observation. When bees are laden they do not flit from flower to flower, or attempt to trace any mazy clue; they fly direct in the upper regions of the air, often at a height of twenty to forty feet from the ground, and in high summer they do the same on their journey outward, clearing the causeway, mounting upward into the azure, and darting, by a direct flight, towards the region they intend to explore for sweets. Nevertheless, Rogers was near the mark, though only in a poetic sense, for assuredly the sense of smell is that on which bees are most dependent for guidance in their outdoor work, as the sense of touch is, no doubt, the talisman which guides them within.

You have heard of the painter (some antediluvian Lance) who represented peaches and pears with such fidelity that the birds came and pecked at them, spoiling the picture and poisoning themselves with paint. Well, it occurred to me last summer to amuse myself by ascertaining, if possible, whether bees were attracted to flowers by their bright colours, or at least of ascertaining whether they could distinguish real flowers from counterfeits. So I got a quantity of artificial flowers of the best possible make; they were of various kinds and colours, and I took care to secure amongst them some tolerably good imitations of the blossoms of clover, and Salvia nemorosa, two of the best bee flowers. I planted these near a convenient spot, both to attract the bees and to allow of constant observation.

I thought if the hive bees passed them by—for hive bees rarely touch the most tempting flowers near the hive—it might at least happen that a red-hipped humble bee would probe their empty nectaries and make a fool of himself for my diversion. I patiently bear the shame that overwhelmed me, because the bees are the gainers thereby; I will humble myself that they may be exalted, and confess that the trick failed most miserably. No hive or humble bee, no butterfly or moth ever visited one of my paper or calico deceptions; I sat for hours watching; I tried my own skill in imitating flowers which the bees were already busy at, but all to no purpose, they treated my baits with proper scorn, and told me they were not to be caught with such absurdities. But it at last struck me that if colour did not attract them, odour might; and so I dropped into a number of flowers a little clean liquid honey, and within a few minutes it was smelt out, abstracted, and carried away, and, with the departure of one bee laden, two or three empty ones came to take their place, and the very worst of the calico flowers was as much favoured as the best imitations, so long as they afforded a store of nectar. It is, no doubt, the odours of flowers that attract bees to them and enable them to distinguish those that afford a harvest from those that are destitute of honey. It is, no doubt, the odour also that guides a bee to the flowers of the same genus or family of plants, and renders for the time the flowers of other genera or families obnoxious; for if a bee begins the day by visiting a cruciferous plant, such as rape or mustard, it continues to work among rape or mustard flowers throughout the day, and will not alight upon a salvia or a clover blossom. It is this habit of visiting during an excursion only the flowers of the same family, that renders the bee so expert and efficient a fertilizer, for the pollen she gathers by brushing the stamens to probe the nectary is diffused where it will exercise a vital influence—that is, among stigmas of flowers of the same race, and oftentimes of the same species. If you observe bees going in laden with pollen, you will never see mixtures; one has red pollen, another yellow, another amber, another white, and so all day long; and when they come dusted all over, as often happens when the thorns are in bloom, the dust is homogeneous. It is no mixture of indiscriminate and unbotanical collectings.

It has been the fashion of late to test the wisdom of Shakespeare's analysis by particular references to historical, theological, literary, and scientific subjects in his works. Look at his references to bees, and wonder. Virgil's fourth Georgic is of course, with all its absurdities, the corner-stone of apiarian literature; but Shakespeare knew the bees as well as he knew mankind, and when he deduces from their history a parallel or

a metaphor or comparison, it has the vitality of observation in it; he had learnt wisdom from the hives of Warwickshire as the Indian Cuma discovered how to send his love-shafts to the heart by stringing his bow with singing bees. I like to compare Shakespeare's utterances with facts, where such comparisons are possible, and his bee lore will always endure the test and prove him master of the hive as well as of the heart. What is there of descriptive poetry to equal the brilliant summary of the social economy of the hive which occurs in "Henry V." (Act i. s. 2), where it only needs to substitute "Queen" for "King," to render the description strictly scientific? In other passages there are the most distinctive evidences of Shakespeare's minuteness of observation, which only those familiar with bees know how to value fairly. In the second part of "King Henry VI." (Act. iii. s. 2), Warwick describes—

> "The commons, like an angry hive of bees
> That want their leader, scatter up and down,
> And care not who they sting in his revenge."

A faithful picture of an enraged colony of bees such as I have seen and attempted to describe,* not thinking at the time how it had been done already by one stroke of the pen by the master-mind of the world.

Among the instincts and habits of bees there is scarcely anything so noticeable as the extent to which they are influenced by the varying conditions of temperature, the hygrometric condition of the atmosphere, and the phases indicated by the barometer. Virgil must have noticed this frequently, how a bright warm day at any season brings them out in myriads, how rain depresses them, and even a passing cloud will sometimes terrify them, so that in a few minutes there will be a general rush homewards, though perhaps there is not the smallest probability of a fall of rain. Do you remember the lines in the fourth Georgic, beginning, "At liquidi fontes" (ver. 18—24), where—

> — cum prima novi ducent examina reges
> Vere *suo*, ludetque favis emissa juventus;

gives a most picturesque idea of the awakening of the hive at the opening of the summer, when flowers abound and the young bees rush forth in the spring of their life to mingle in the vast concerns that make the business of the hive complete. The first simile on this head is taken from the life of the honey-bee, and Virgil, Tasso, and Milton have sucessively imitated the passage. The only method of testing the value of such passages is to bring them to the hive, and then, what

* *Vide* "Apis, a Tragedy," *Cottage Gardener*, vol. xvii. p. 243.

strange confusions of fact and fiction do we discover, and how easily do we trace where the thread of original observation was snapped asunder, and the poet was governed by hearsay in matters that might have been determined with less difficulty than the learning of a tradition. Virgil's ridiculous recipe for the production of a swarm of bees from a carcass has a parallel in Varro, and both are paralleled again in the story of Samson. A man who has hived bees into buckets and flour tubs, and seen them hive themselves in a hole in the wall, and, in fact, show a readiness to enter any dry dark place at swarming-time, sees no difficulty in adding to, or subtracting from, these narratives, in order to restore to them their original complexion of sober truths. Virgil's case is, no doubt, a sheer confounding of bees and blow-flies, an unpardonable error in so observant and picturesque a poet. Samson's adventure simply requires us to supply a few imaginary particulars. Samson slew a lion on his way to Timnath. He left the carcass on the ground, the vultures and hawks soon stripped it of flesh, the heat of the sun, the myriads of insects, and other causes, soon rendered it a bleached skeleton, such as the traveller in the East is familiar with, especially in camel tracks. Into this clean skeleton a swarm of bees hived themselves, and a very excellent place of refuge it would be for them in such a climate as Timnath. *After a time* Samson returned, "and behold there was a swarm of bees and honey in the carcass of the lion" (Judges xiv. 8). My bees are now building fine combs in the open air under the front edge of the hive-board, and if I cut those combs away they will begin again to build more. They must have room; they must have air; if their hives do not afford them all they want, they are not slow in devising schemes of their own to meet any emergency.

It is well known that bees are irritable at the latter end of the summer, when they have a large store of honey, and that then is a dangerous time to meddle with them. Yet, even then, the same caution and presence of mind needful at other seasons suffice to protect the apiarian better than any bee-harness. To show how simple and safe a matter bee-keeping is, when all foolish fears are banished from the mind, and the apiarian exercises a firm hand and a cool judgment in all his transactions with them, I will tell you how I obtain my annual honey harvests, and I shall, perhaps, add thereby something to the stores of apiarian literature. From the beginning of my bee career, I invariably followed the plan of smoking the bees out of the honey-box by means of tobacco, a long and tedious and unpleasant operation. At last it struck me, that as I could handle bees at any time with impunity, why should

I not take the honey without the aid of narcotics and all the attendant puffing and fussing. So, having driven home the slides between the bars, I carefully lifted up a honey-box, which was then quite full, and carried it away without turning it over, and set it down with the edges resting on a couple of bricks. A smart tap dislodged a mass of bees; these fell on the ground, and began at once to return to the mouth of the hive. The honey-box was then carried a little distance away, placed on a couple of bricks, and the process of tapping repeated. It was then taken into the house and placed on a board at one of the windows. After remaining thus for ten minutes, the window was opened and the box was tilted on edge with a pencil, so as to afford egress to the bees. Alarmed at their imprisonment, these rushed forth by means of the open window, and in a few minutes the honey-box was nearly empty. The few drones and incapables left were whisked out with a feather, and thus, without losing a bee, without any experience of their anger, without disconcerting the colony, the harvest was obtained in a manner almost as simple as the purchase of a loaf. I have followed this method for five years, and have several times during that period manipulated in the presence of bee-keepers, dependent on tobacco and fungus, and the verdict has been in favour of this method, making a clear end of all the troubles, real or imaginary, that beset bee-keeping.

ATTEMPTS TO FORETELL THE WEATHER.

Admiral Fitzroy has brought his system of *forecasts* to a very creditable amount of perfection, by pursuing a method that may ultimately take into account a greater variety of indicating circumstances, but which is founded essentially upon a knowledge of the order in which weather changes traverse certain portions of the globe. Barometers and thermometers give local indications of changes of pressure and temperature that may be expected to be followed by certain results. He therefore notices the movements of these instruments in a more scientific way than was commonly done in old times, and by help of the telegraph he ascertains from well-chosen localities, both what weather they have at the moment his message is sent off, and what their barometers and thermometers are about. Putting all his information together, he indicates what sort of weather is on the road to us, and whose arrival we may expect, unless local conditions interfere. A Fitzroy announcement

partakes of the character of a mercantile letter of advice, but instead of "shipped from so and so, so many chests of tea," it tells that air currents, dry or moist, have passed over certain places, and may be expected to visit other localities as soon as time permits. Labours of this kind are not likely to lose their value through the progress of science, because the discovery of general laws regulating atmospheric changes would not do away with the necessity for watching as accurately as possible every indication of the modifications the effect would experience at any particular place. If, for example, Mr. Balfour Stewart's theory of the action of the planets on the sun should be verified by observation and experience, we might calculate in advance when there would be a strong outpouring of heat; but a great many circumstances, arising from a multiplicity of causes, might help to decide when and to what extent any particular locality would be warmed.

That weather changes occur without law, no one believes, and if by law, why should not the law be discovered? The doubt which many have felt has arisen from a belief that too many forces co-operated, and too many incidents modified their action, to admit of calculation with the precision that science requires, or that utility needs. To this it might be replied that there may be certain conditions sufficiently dominant to ensure a general or average result of a particular kind whenever they prevail, and that although we may not be able to say that London will be visited by a rain or thunder-storm on any particular day next year or the year after, we may find reason for predicting that on, or close to such a date, a rain fall or electric disturbance will take place within the limits of a meteorological district of which the metropolis may form a part. No one can deny the possibility, or even the probability, of attaining to this class of information, and if it is reached at all, it is very likely to be arrived at by the method of watching coincidences, and thus ascertaining an empirical law.

Captain Saxby is very confident that he has discovered a great weather law, according to which "the moon never seems to cross the earth's equator without there occurring at the same time a palpable and unmistakeable change in the weather. Such changes most commonly are accompanied either by strong winds, gales, sudden frost, sudden thaw, sudden calms or other certain interruptions of the weather according to the season."*

Mr. Pearce and the "Astro-meteorologists," as they call themselves, not only make very positive assertions that weather changes accompany or follow certain planetary configurations, but they plunge head over ears into theories by which they

* *Saxby's Weather System*, p. 7. Longmans.

fancy they explain how the changes are produced. Their assertion is that Uranus, Saturn, Jupiter, Mars, Venus, and Mercury are chiefly concerned in determining terrestrial weather, and that "they are more potent when they are on the equator, (in other words, crossing the line); when at their extreme declinations; when in conjunction with, and in opposition to the sun (analogous to new and full moon); and when at 90° distant from that body, answering to the moon's quarter."*

Now it would seem, at first sight, a very easy thing to settle the merits of the rival systems of the Lunarians and the Astro-meteorologists. It is easy to ascertain from the Nautical Almanack when the moon and the planets were in certain positions in times past, and to look at the weather records for the same periods. It is also easy to see when the celestial bodies alluded to will be in positions that, according to Saxby or Pearce, ought to produce wet, cold, storms, or calms, as the case may be. But it is far from easy for one not belonging to the school of the weather prophets to agree with them as to the mode of investigation. Both the Lunarists and the Astro-meteorologists appeal to certain cases in which the facts coincided with their predictions; but the latter, who are the most daring in their assertions, often think themselves right when other folks might consider them wrong. If rain is predicted in a weather prophecy published for London, and a fall takes place in Cumberland, while London remains dry, the weather prophet is satisfied, although his information has not helped the Londoner in the least degree. If he predicts heat, and it turns out that the days are not warm, but that the nights are so, and thus the average temperature is high, he thinks he has caught a "coincidence," and makes the most of it. Next time, his prediction of warm weather is, according to his belief, equally fulfilled, although the circumstances are different, and the days are really hot. We mention these things to show how necessary it is that the prophets should be tied down to a very definite statement. If they prophecy heat or cold, they should tell us exactly what they mean; and as hot days may be followed by cold nights, or cold days by warm nights, they should be called upon to distinguish between these two very different modes by which a similar average temperature may be obtained. With respect to rainfall, thunder, etc., to say there will be one or the other, without specifying the exact portion of the globe in which they are expected to occur, is to be wanting in precision; and if the motions of the earth and planets determine the march of weather changes, the prophets should inform us of the order in which the alleged astral influences will affect given zones.

* *The Weather Guide, a Concise Exposition of Astro-Meteorology.* By Alfred J. Pearce. Simpkin.

According to the *Record* of the Astro-Meteorological Society, "every astro-meteorologist *believes and knows*" many startling propositions. Among them he is quite certain "that *oxygen*, the red ray of solar light, and positive electricity, are *identical*; and that the *blue ray* of light, or *nitrogen*, is equivalent to *negative electricity*." These are curious things for Astro-meteorologists to *believe*, and very remarkable things for them to *know*. In pursuing their speculations, Jupiter is assumed to exercise an important chemical and electrical influence on the earth's atmosphere, because his light, of which we get very little, is found to be fourteen times richer in chemical rays than an equal quantity of moonlight. Assertions and reasoning of this kind will fully account for the neglect of Astro-meteorology, of which Mr. Pearce complains; and our faith is not strengthened by the declaration "that *all* coincidences, when they are sufficiently frequent to be removed from the confines of chance, prove that the two things that coincide are either 'cause and effect,' or are the 'effects of a common cause.'"

Now it is perfectly possible that two or more series of events, not casually connected with each other, and not springing from even a similar cause, may yet coincide at certain recurring points of their courses. Coincidences of this kind may be called effects of *chance;* but the idea of chance is not eliminated because such coincidences recur any given number of times, nor even by their perpetual recurrence.

The observation of a sufficient number of coincidences may justify the acceptance of an empirical law, according to which we may, with approximate safety, predict that, when one of the events happen, the other will accompany or follow. To get beyond a merely empirical law of this kind, we require the support of another series of inductions, or of as many more series as we can obtain. Thus the law of gravitation is evidenced by as many independent series of facts as there are separate sets of bodies gravitating towards each other.

The mistake made by Mr. Pearce is in attaching undue value to *a single and unsupported* series of coincidences, however numerous. We are not justified in asserting that any incidents are the physical causes of certain other incidents that follow them, until we have arrived at collateral reasons for believing that the assumed causes *uniformly* exert the kind of action required, and that they do so, in the cases under investigation, with sufficient force to account for the results that we trace.

A little reflection will show that it is not every concurrence of events that can form the sort of coincidence, the multiplication of which will logically lead up to an empirical law. Suppose, for example, that any coincidence seeker should tell us that rain

was the cause of the striking of city clocks, and he supported his assertion by stating that it never rained in London for sixty-five successive minutes without several clocks recording the hour, we should admit his facts, but reject his hypothesis. In like manner, the Lunarians and the Astro-meteorologists may be often right in their assertion that certain events follow each other; but, like the coincidence seeker just mentioned, they may not take adequate account of the number of times in which one of their alleged connected events occurs without the appearance of the other.

We know nothing of physical causes except by noticing cases of what appear to be invariable and necessary sequence. After a certain amount of experience we assume the invariability and the necessity; and we do so most readily when one set of experiences is backed up and supported by other sets of experiences. Thus watching for coincidences and sequences is a necessary process of scientific discovery. If the Astro-meteorologists did not make too much pretension, and evince too much haste in propounding theories, their labours would be held in much higher respect by scientific men. Inside their own circle the faith may be absolute, but the outer world is apt to distrust the investigation of men who start with the assumption that they know all about the thing to be ascertained and proved. It is also unfortunate that they talk about electrical actions when they cannot refer to an ascertained electrical fact that supports their hypothesis. In the earlier days of electrical science, every unknown agency that did anything wonderful was supposed to be electric. When spirit-rappers affirm that mahogany sideboards walk across the room, certain of their believers suppose electricity has endowed the furniture with locomotive powers; and a few years ago, when repeated fires broke out in a certain house without apparent cause, electricity got the credit of the incendiary deeds. In such ascriptions of all sorts of incidents to the power of electricity there is nothing logical or scientific; and when any one, ignorant of the real physical cause of an event, and not having traced any of the specific characters of electrical action to be connected with it, still asserts that it is, or may be electrical, we can only take such an expression as an indication of running mentally wild.

Captain Saxby is far more prudent than the Astro-meteorologists, because he indulges in less unproved speculation and baseless assertion. That the moon does affect our weather very powerfully has been believed in all times, and the supposition is highly probable. It must, however, be admitted that many theories of lunar action have not been borne out by experience, and doubts must be entertained whether any particular hypothesis be true. The subject is well worth investigation by the

method of seeking for coincidences; and those who engage in the pursuit ought, in common fairness, to pay due attention to the statements which Captain Saxby supplies. We may also say that, after rejecting all the hypothetical matter put forth by the Astro-meteorologists, it may be well worth while to compare weather changes with planetary movements, and, in so doing, to take into account the alleged experience of Mr. Pearce and his friends. We use the term alleged experience advisedly, and without the slightest doubt of the integrity of the Astro-meteorologists. The value of their experience depends upon the care they have taken to eliminate error; and while we do not doubt their good faith, they have as yet failed to convince us that they have exercised due caution in their search for truth.

Many of our readers may think that Astro-meteorology has not entitled itself to the serious notice we have taken of it. This may be true if it be regarded simply as a philosophy; but we must protest against neglecting to watch for coincidences between planetary motions and weather changes, because certain very enthusiastic gentlemen have brought ridicule upon the process through hastiness and over zeal.

COLOURS OF STARS.—CLUSTERS AND NEBULÆ.

BY THE REV. T. W. WEBB, A.M., F.R.A.S.

The interesting subject of Star Colours may yet admit of a little further elucidation; more especially since a recent discovery, of a peculiarly brilliant and striking character, gives fair promise of our gaining some degree of acquaintance with the constitution of the stars, notwithstanding their inexpressible remoteness. It is but reasonable to suppose that the colour of stellar light must depend, at least to a considerable extent, upon the nature of the incandescent materials by which we conceive it to be emitted; and, strange as it may appear, we are now in a position to guess, with a measure of confidence which would a short time ago have been incredible, at some of the elements which enter into the composition of the starry photospheres. Our readers will be familiar with the extraordinary results which have been deduced by MM. Kirchhoff and Bunsen from their examination of the solar light by what is now termed "spectrum analysis." They may be thus summed up. The spectra formed by the light of intensely-heated bodies usually exhibit a set of transverse lines or bands, and these appear to be constant as to number and position with

respect to the same material—at least at similar temperatures; so that the aspect of the lines of any spectrum, when analyzed by a suitable apparatus, is an unerring indication, so far as is yet known, of the presence of the material to which that set of lines belongs. Hence, when among the multitude of lines contained in the solar spectrum, we are able to identify those which are known to belong to the spectra of certain elementary substances, we are naturally led to infer the presence of those substances in the body of the sun.* Of course, before anything like certainty can be ascribed to this inference, it must be demonstrated that the lines appertaining to every element are unchangeable, at least under any conditions fairly presumable upon the solar surface; and that one element alone is associated with any given set of lines. The proof of the former of these premises would be difficult; of the latter, while so many unknown elements may exist, impossible. But we must be content in this, as in many similar cases, with inevitable imperfection, and accept willingly the nearest approximation within our reach, to a truth in its own nature apparently beyond demonstration. This daring investigation into the nature of the sun has now been for some time before the public, and though it has been assailed with vigour, it seems to be gaining ground, and likely to keep it: and it is truly interesting to learn that by the most praiseworthy researches of two of our own countrymen it has received an extension which might indeed have been readily anticipated, but could only be effected through the means of costly and laborious experiment. We now have begun to study the constitution of the stars. Secchi and others had already commenced this investigation, but, so far as we know, no direct and simultaneous comparison had previously been made of the lines visible in the various stellar spectra, with those produced by terrestrial elements. This extremely delicate and difficult research has been recently commenced, with most gratifying results, by the joint efforts of Mr. Huggins and Dr. Miller. They entered upon their task, aided by an 8-inch object-glass of Alvan Clark, in January, 1862, and the conclusions which they have already drawn are truly marvellous ones, and such as reflect the highest honour upon the observers. There seems as much reason to believe in the existence of the metallic elements sodium, magnesium, calcium, iron, and bismuth in the atmosphere of *Betelgeuse* as in that of the sun! To these *Aldebaran* adds hydrogen, tellurium, antimony, and mercury. *Sirius* gives us sodium (which indeed seems to exist everywhere), magnesium, hydrogen, and probably iron. *Wega*, sodium, magnesium, and hydrogen. Other spectra have been

* The question of absorption has been intentionally omitted, for the sake of greater perspicuity.

examined and measured, and lines are found in all cases where there is sufficient intensity of light. The conclusion drawn by these observers is truly magnificent—that since the elements most widely diffused throughout the stars, so far as they have been examined, are among those most clearly connected with the existence of life upon the earth, "these observations give a basis of some probability to that which has heretofore been *but pure speculation*—namely, that at least the brighter stars are like the sun, the upholding and energizing centres of systems of worlds adapted to be the abodes of living beings." A conclusion, to which it may fairly be added, that the diversity everywhere traceable, amid general similarity, in the character of the analyzed light of the sun and stars, would lead us to infer no less diversity in the nature of the life, both vegetable and animal, dependent upon it. And what an overwhelmingly vast conception is that thus opened to us, though only by a single glimpse, of the riches of creation!

"O Lord, how manifold are Thy works!
In wisdom hast Thou made them all."

We must return, however, to the matter in hand, as to which another remark by the same observers is much to the purpose, that the colours of the stars, though not the alleged variations of those colours, receive an explanation from these researches; the number of dark lines occurring in any part of the spectrum weakening, of course, the colour of that particular space, and giving prominence to the hues of the portions not similarly interrupted. This explanation presupposes an identity in the composition of all light otherwise than as it may be interfered with by the existence of non-luminous bands denoting the presence of certain elements. But there is no proof of this hypothesis. It may be equally probable, as Smyth has supposed, "that the light of some of the stars is absolutely of a distinct nature, and radically of a different composition, to that of the sun." And amid all the diversity of creation, there is no antecedent impossibility in the idea that as our solar light is compounded of three colours, other suns may exist in which two of them, or one only, may be found. And though in the solar spectrum and all others with which we are familiar, the three primary tints preserve their relative refrangibility, and consequent position, it by no means follows that other spectra may not exist in all the boundless realms of creation, where other arrangements and proportions might be found, and possibly where colours may be developed hitherto unseen by mortal eye. For though we can conceive none which are not our three primaries, or modifications of them, yet as colour seems to depend upon the adaptation of the undulations of

light to the receptive power of our nervous system, with a varied organization it might not be impossible to perceive hues different from those to which alone our optic nerve is capable of responding. But we are wandering too far into the regions of mere speculation, and in some danger of losing sight of our original subject, which was not so much the colour of stellar light in general, as its liability to change. It would be premature to conjecture whether, supposing that change established as a fact, spectrum analysis may be able to give us any information as to its cause; but there seems little expectation from any other quarter. One curious hypothesis however deserves to be mentioned from its ingenuity rather than its verisimilitude. It has been suggested by Sestini that the components of double stars may change their colours periodically, in consequence of the corresponding alteration in the direction of their orbital velocity towards or from our eye. For since it is supposed that colour consists of very minute undulations or vibrations, and that these are much more rapid at the violet than the red end of the spectrum, it would follow that any change in the rate of the vibrations would be attended with a corresponding alteration of colour. Now it is evident that the motion of the radiant body towards or from the eye, provided that it is swift enough to bear a certain proportion to the velocity of light, must produce an apparent increase or diminution in the rapidity of the vibrations, since, though their actual rate remains unchanged, more or fewer of them will reach the eye in the same time, according as the motion of their source concurs with or is opposite to that of light; and hence would result a corresponding change of colour. There can be no doubt as to the correctness of this inference, which is fully borne out by a curious analogy. It has been pointed out by Mr. Scott Russell that in railway travelling the pitch of sounds towards which we are rapidly approaching—for instance, the whistle of an engine coming in the contrary direction—will be raised or sharpened from an increased number of the vibrations which constitute sound entering the ear in the same time; at the moment of nearest approach the sound will descend to its true pitch, and subsequently grow flatter as the distance widens again. A little attention will enable us to try for ourselves this interesting experiment, which presents, in regard to another of our senses, a perfect correspondence with Sestini's idea as to colour. But notwithstanding this collateral support, his hypothesis is open to grave objection. The velocity of railway transit bears a much larger proportion to that of sound, than any probable orbital velocity of a star does to that of light, for, as Smyth has remarked, though we have no right to assume that the velocity of stellar is identical with that of

solar light, yet it would be difficult to conceive any light slow enough to admit of the possibility of the explanation. And besides this, it is inconsistent with experience; since many of the colours of binary pairs, revolving rapidly, and favourably situated for such an observation, continue unchanged. It is possible, indeed, as the admiral has shown, that a newly-created or recently-extinguished star might show a succession of colours, from the different times in which the different undulations would probably reach us; all merging at last into white, or disappearing in darkness. Or if a star were conceived only to exist for a very short time, we might have all the hues of the spectrum in succession, without any white combination, though possibly with intervals of invisibility corresponding with the non-luminous bands already referred to, which traverse in great numbers the breadth of the spectrum. But in the case of a star shining from time immemorial, its colour, so far as it depends upon any such cause, would be nearly white. The same eminent astronomer has pointed out that on this supposition of unequal velocity in the several colours—which it will be remembered is merely a hypothesis, though a very probable one—a change of brightness alone in a star might occasion a change of colour, for "the strong blue of a bright epoch arriving with the faint red of a dull period, will make blue appear to us as the predominating colour—will cause indeed the star's light to appear decidedly blue at one time, and, *mutatis mutandis*, red at another, although all the while the star's colour may not really have altered at all; but may have been really, and would have appeared to observers close by, as white as ever, varying only in quantity and not in quality." The celebrated "variable" *Algol* was examined by Arago in order to test this idea, but without success; the admiral, however, remarks that other stars might be picked out where the natural circumstances are more promising, and that more accurate modes of comparison than that adopted by Arago might be employed.* Mr. Hind is of opinion that he has detected such alterations, and that several variable stars are blue in increasing, become yellow after their maximum, and are red in their decrease; and I am indebted to the kindness of Mr. Knott, to whom our readers are thus often laid under much obligation, for the following instances: 1848, Sept. 3, a star, R. A. 5h. 34·8m., D. N. 21° 6′ (1800), was entered by Hind as "very red;" it was closely watched for variable light without change till 1850, Nov. 14, when it was found "decidedly bluish, the red tinge having vanished entirely." A second is a "variable,"

* Students who are desirous of examining this subject more in detail, and as developed by the hand of a master, may receive full satisfaction in the *Ædes Hartwellianæ* and *Speculum Hartwellianum*.

R. A. 0h. 58m. 56s., D.N. 22° 55′ (1860), which both Hind and Pogson concur in representing as changing through blue, yellow, and red; a third variable, R. A. 7h. 34m. 38s., D. N. 23° 46′, is red and blue at different times; a fourth, R. A. 7h. 40m. 54s., D. N. 24° 5′, still more decidedly so. Should future experience prove this phenomenon to be of frequent occurrence, we may be able to infer with some probability that a change of colour in such cases may have its cause in variations of brightness; but this will leave us as far as ever from an unravelling of the mystery whence such changes can arise, so long as the amount of light continues the same. Here we are as much at fault as ever.

The first step towards the solution of this latter problem, if indeed it is not an irresolvable one, is obviously the more distinct establishment of the fact whence it springs; for hitherto it may be said that such changes, except in the case of variable stars, have been rather suspected than proved. The evidence as to *Sirius* is strong; but its light may also have varied. Schmidt's idea that *Arcturus* has of late years been losing its ruddiness does not seem confirmed by other observers. The curious discrepancies between the colours of many double stars as given by Smith and Sestini seem rather referable to "personal chromatic equation," or in plain words, peculiarity of vision on the part of the latter; and a similar remark may be made as to the colours given by some other observers. There is, however, one pair now conveniently situated for observation, to which attention has been recently directed, and which ought to be carefully watched by those who are interested in these studies. This is the very beautiful double star 95 *Herculis*—No. 40 in our list (*see* INTELLECTUAL OBSERVER, Sept. 1862, p. 136). During 1828, 1829, and 1832, Σ (the astronomical symbol for W. Struve, or Struve I.) always found the *p*, or smaller of the two, reddish yellow, the *f* greenish yellow,—remarking on the singularity of a difference so much greater in colour than in brightness. Smyth made them cherry red and greenish, 1833·8; Sestini, both gold yellow, 1844·5; Smyth again reddish and pale green, 1851·3; his son, C. Piazzi Smyth (Astronomer Royal for Scotland), both white, July 29, or both yellow with tinge of bluish green, Aug. 4, 1856, at a height of 8870 feet upon the peak of Teneriffe; an estimate confirmed by the eyes of some Spanish visitors: the admiral, however, shortly afterwards found them cherry red and apple green, just as they were nearly a century before. With this description Dawes very nearly agreed, and four other observers, about the middle of 1857. Captain Higgens, in the autumn of 1862, found the colours as given in the Bedford Catalogue (apple green and cherry red): 1863, April 23, he

says, "The bright hues were not there, and greenish-white and pinkish-white were all I could affirm. May 10, hues more faint; I could only record them as dull white, both A and B. Aug. 1, A, greenish-white; B, yellowish: both changing nightly till Aug. 12, when they showed as in the *Cycle*—A, apple green; B, cherry red. A first showed signs of deepening colour, the hues becoming more apparent every night; B changing from yellow to red more rapidly. The change," he adds, "was very palpable after an interval of three or four nights."

Our readers will no doubt avail themselves of the present season to examine this "crucial instance," as Smyth calls it, "of sidereal colour-changing." I have observed it but once with my present instrument—on Aug. 6—when I found it rosy and greenish, but thought the red more decided than the green. Out of six eye-pieces, of different kinds, ranging from 111 to 451, one only, of Steinheil's construction, failed to show the difference of colour.

It would be well if those who feel an interest in the inquiry would note down—with the date of the observation—any remarkable colour in a star, single or associated, which may pass through their field of view, and which can be identified on future occasions. This will involve very little trouble, especially when it has become familiar to us; the perception of such colours, cultivated by practice, will become at once more sensitive and more accurate; and the record of our observations may ultimately be found of considerable value, while we shall be at any rate sufficiently compensated by the beauty and variety which we shall thus discover in every quarter of the heavens.

A few remarks on the subject of the distribution of star-colour will close this already extended discussion.

The statistics of colour occurring in pairs, as determined by Struve, will be found in Intellectual Observer, March, 1862, p. 149. They lead at once to the conclusion that although there is a preponderance, as we have already pointed out, of the more refrangible end of the spectrum among the minuter components, the reverse is decidedly the case with the brighter ones; and this is so evident, not merely among associated but insulated stars, that Sir John Herschel has said that "no green or blue star (of any decided hue) has, we believe, ever been noticed unassociated with a companion brighter than itself." So general an expression may not accord in its fullest sense with the results of other observers; yet there can be no doubt of the general predominance, next after white, of yellow, orange, and ruddy hues. The ancients recognized only white and red stars among those visible without a

telescope, and Arago expresses an opinion that among the sixty or eighty thousand isolated stars which have been catalogued, no other designations except white, red, and yellow are to be found. Here we seem to trace the existence of some unknown general law. Again, it may be remarked that stars of a deep fiery aspect, or even a ruby or actually sanguine hue, are not very uncommon, while an equal intensity in green or blue is scarcely to be met with. These crimson or sanguine stars, however, do not attain any conspicuous brightness; and it is doubtful whether this can be accounted for on the general principles of average distribution, as their whole number is not great, or whether it may be connected in some occult way with actual size. At any rate, no instance, so far as I am aware, of crimson light occurs among the multitude of stars above the 8th magnitude; Herschel I.'s "garnet star" (*μ Cephei*, 6 mag., variable), by no means answering the description. *Antares*, the leader of the ruddy class, shows little more than a rosy orange; *Aldebaran*, *Betelgeuse*, and others are still less intense. Another curious fact is, that such sanguine stars are, without exception, so far as is hitherto known, insulated—no doubt a significant fact, could its interpretation be only found. And so probably is that mentioned by Herschel II., that it is no uncommon thing to find a very red star much brighter than the rest, occupying a conspicuous situation in a cluster; and that described by Secchi, that among the spiral curves, or radiating figures, in which the larger galactic stars are so marvellously arranged, there is usually in the centre of the radiation, or at the starting-point of the spiral, a redder as well as larger star. "Il est impossible de croire que telle distribution soit accidentelle." And still more convincingly must this conclusion be drawn, when closely aggregated nebulæ exhibit a general blue light pervading all their constituents, or when a rich globular cluster is all of a pale rose tint, within a concentric border of minute white stars. Here it would be blindness indeed not to acknowledge the presence of some law, not the less real, because at present it escapes our grasp, or because the development of it may possibly be reserved for a future state of existence.

CLUSTERS AND NEBULÆ.

The earlier closing in of our evenings renders it now pleasant as well as practicable to return to the search after objects requiring a dark background, and, before it sinks too low, we will begin with—

19. *The Cluster in Sobieski's Shield.* I venture to call it so, because—though according to our standard authority, the Bedford Catalogue, it belongs to the constellation *Antinous*—it

is not only reckoned to the *Clypeus Sobieskii* by the Star Map of the S. D. U. K., but belongs to it as matter of natural arrangement: for popular purposes, we may disregard in such an instance the entangled boundaries which here, as in other places, tend to so much confusion. The *Clypeus*, or *Scutum Sobieskii*, is one of the constellations formed by Hevelius out of the stars which were strewed about, in unappropriated disorder, amongst the ancient asterisms, and named by him in honour of the glorious deliverance of Vienna from the Turkish siege in 1683 by the chivalry of Poland, headed by their heroic sovereign John III., of the house of Sobieski; and, in general, though not strictly accurate terms, it may be described as the mass of nebulous light between *Al Tair* and the S.W. horizon, where the galaxy seems to approach especially near to our system. At the upper *following* termination (if such a word may be applied) of this luminous area, we shall perceive three stars near together, in a gentle curve bending downwards to the right, λ *Antinoi*, 3 mag., and 12 and 19 *Aquilæ*, 5 mag.—the continuation of this curve points out our object at a short distance, or it may be found 2° *s*, a little *f*, from 6 *Aquilæ*, a 5 mag. star, at the upper *preceding* termination of the nebulous mass. It is 11 M., and is described by Smyth as a splendid cluster, somewhat like a flight of wild ducks. H. speaks of it as "a beautiful irregularly round cluster, 10′ or 12′ diameter; the stars are all 11 m. except one = 9 mag.—examined with high magnifiers (I have often viewed it with 800 and even 1200) it is broken into five or six distinct groups, with rifts or cracks between them." It may be gratifying to the possessors of far smaller instruments than H.'s 18-inch reflector to know that this curious structure is quite within their reach, as I perceived it with powers of 65 and 111 upon my 5¼-inch aperture, though not in the least aware that it had been noticed previously. The bright star a little *sf* from the centre is rated 8 mag. by Smyth, as well as an open pair lying clear of the cluster in that direction. This fine object was discovered as a small obscure spot by Kirch in 1681, and first resolved into stars by Derham with an 8 feet reflector. The galaxy in the vicinity is fairly resolvable with my achromatic, the field being all "stippled" over with points of light. As Smyth says of this region, "the wonderful quantity of suns profusely scattered about here would be confounding, but for their increasing our reverence of the Omnipotent Creator, by revealing to us the immensity of the creation. * * * So great is the number of stars in some parts of this Via Lactea, that H observed 588 of them in his telescope at the same time; and they continued equally numerous for a quarter of an hour. In a space about 10° long and 2½° wide, he computed that there

were no fewer than 258,000 stars. Carrying this view into adjoining regions, words and figures necessarily fail, for the powers of mind falter in such vast and awful conceptions."

We will now take the most celebrated and best known of all the globular clusters, and certainly the most beautiful in the smaller class of instruments. It is—

20. *The Great Cluster in Hercules*, or 13 M. To find it, we must run a line from *Wega* to *Gemma* (*α Coronæ*). This will first fall upon three stars near together, which, reckoning from *Wega*, are *ρ Herculis*, 4 mag. (No. 39 of our Double Stars; INTELLECTUAL OBSERVER, Sept., 1862, p. 136), 69 *Herc.*, 4 mag., and *π Herc.*, 3 mag. The line will then pass, at somewhere about half its length, midway between two 3 mag. stars lying at some distance above and below it, *η Herculis* above, *ζ* below. Between these two again, but about ⅓rd of the way from *η*, lies our cluster. Halley, who discovered it in 1714, says, "This is but a little patch, but it shows itself to the naked eye when the sky is serene and the moon absent." The finder will, of course, catch it at once; and in small instruments, and with low powers, it will appear as a very beautiful bright nebula, while its resolution will be complete in proportion to the aperture and magnifier employed. Messier satisfied himself, with a power of 60 in a 4 foot Newtonian, that it contained no star! It is said by Arago to have counted—he must have meant, guessed at—more than 14,000 with the 40 foot telescope. Its starry nature was very evident with my $3\frac{7}{10}$ inches, and with my present aperture its resolution under a high power, though still nebulous and requiring more light, is very fine. Sir J. Herschel describes it as "very gradually much brighter in the middle; stars 10 to 15 mag., of which there must be thousands; does not come up to a nucleus; has hairy-looking curvilinear branches:" and, in another observation, "irregularly round, with scattered stars in streaky masses and lines; excessively condensed to a perfect blaze; stars 11 to 20 mag." (the extreme of his scale), "7′ or 8′ diameter. Most magnificent object. The state of compression indicates a globular form not much denser at the centre." The curvilinear arrangement here spoken of is curious. We have referred elsewhere to its existence as pointed out by Secchi in the galaxy; and the Earl of Rosse, to whose optical power even the central condensation has given way, observes, with reference to the inquiry whether the spiral form perceived in certain nebulæ does not indicate a structure quite different from that of any known cluster, that in the exterior stars of some clusters, of which he gives 13 M. as an example, "there appears to be a tendency to an arrangement in curved branches, which cannot well be unreal or accidental." So that we seem

to be tracing the working of some widely diffused law. This object deserves careful study, and in the finest weather, if our instrument gives any hope of its resolution. Sm. remarks that "it is indeed truly glorious, and enlarges on the eye by studious gazing."

Not far from 13 M. we meet with another cluster—

21. *The Second Cluster in Hercules*, or 92 M. It is not quite so easy to find, but well worth the trouble. At some distance from *Wega*, *np*, we notice two conspicuous 2-mag. stars, a few degrees apart, β and γ *Draconis*, marking the Dragon's head, of which β is *p*. About half way from π *Herculis*, mentioned in finding our last object, to β *Draconis*, a little sweeping will discover our cluster, which is a very beautiful brilliant mass, smaller than the last, but considerably brighter and less resolvable, though still evidently of a starry character. Smyth calls it "a large, bright, resolvable cluster, with a very luminous centre and irregular streamy edges," 7′ or 8′ in diameter in H.'s reflectors. It is not referred to in H.'s catalogue. Messier discovered it in 1781.

We now turn to a different quarter of the heavens, in search of a similar object—

22. *The Cluster in Pegasus*, or 15 M. We must refer to our old acquaintance ϵ *Pegasi* (Double Stars, No. 65, Intellectual Observer, Dec., 1862, p. 375). Between it and γ *Delphini* (No. 63), at about one-fourth the distance from the former, the finder will show us four small stars at once in its field, one of which is rather fainter and more hazy than the rest. This is our cluster. It is not unlike 92 M. in size and general appearance, equally beautiful, bright, and condensed in the centre; resolvable, but not resolved, in ordinary instruments; revealing its starry nature in proportion to the stedfastness of the observer's gaze. With a low power the field is a striking one. Sm. describes it as a noble cluster, with stragglers branching from a central blaze, and says, "under a moderate magnifying power, there are many telescopic and several brightish stars in the field, but the accumulated mass is completely insulated, and forcibly strikes the senses as being almost infinitely beyond those apparent *comites*." H. calls it "superb," and speaks of it as "very compressed; irregularly round; very small stars 15 m.—comes up to a perfect blaze in the centre—not the condensation of a homogeneous globe; it has straggling streams of stars, as it were, drawing to a centre—4′ or 5′ diameter." From this reference to the aspect of a homogeneous sphere, compared with the description of 13 M., we may find it interesting to notice the way in which the internal constitution of such clusters may be estimated. If we draw a circle upon a piece of paper to represent the section of a globular space filled

with stars arranged at equal distances throughout, and then intersect it by six or eight parallel straight lines, representing the direction of our sight, since the length of each included line will indicate the number of stars it encounters, and consequently the brightness of the portion it traverses, we shall perceive that the luminosity of such a cluster would increase very rapidly near the edges, but make slow progress towards the centre. If, therefore, we find an object like the present, or the last, exhibiting a very different rate of increasing brilliancy, we are warranted in concluding that it is composed of stars, not equidistant, but centrally compressed in proportion to the abnormal accession of light.

ANGLO-SAXON POTTERY.

BY THOMAS WRIGHT, F.S.A.

(With a Coloured Plate.)

THE importance of ancient pottery as an evidence of the date of deposits with which it is connected and of the people who made it, has been much better understood of late years than formerly; and the pottery itself has been more carefully examined and more accurately classed. We are now well acquainted with many varieties of the pottery manufactured by the Romans in these western provinces of the empire, as well as with that of their successors in the period which in our island we call Anglo-Saxon. The interest of the subject is, indeed, now so great, that a popular article on the principal varieties of ancient pottery will perhaps be from time to time acceptable to the readers of the INTELLECTUAL OBSERVER. We will in the present paper begin with the Anglo-Saxon pottery, which presents one or two points of interest peculiar to itself.

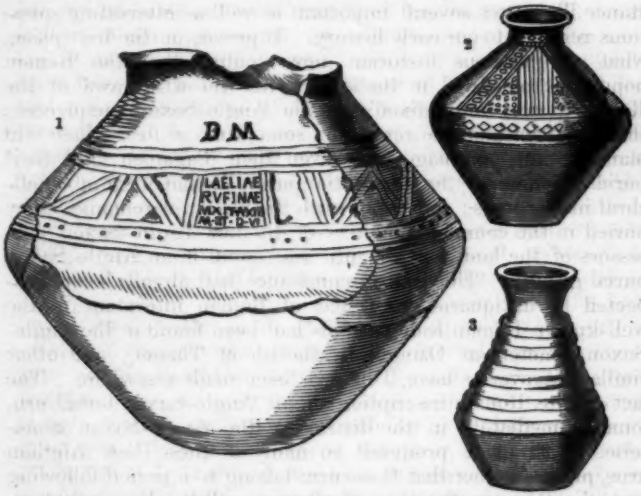
Our knowledge of the antiquities of the early Anglo-Saxon period originated in Kent, where very extensive excavations in the Anglo-Saxon cemeteries scattered over the Kentish downs were made in the last century, under the direction of Bryan Faussett of Heppington, and the Rev. James Douglas, the author of the *Nenia Britannica*, one of the most valuable of the older works on our national antiquities. It was only gradually that the real value of these remains was understood, and it has been only fully appreciated within the present generation. In the Anglo-Saxon graves of Kent, however, the quantity of pottery was not great, and a large proportion of that which was found was of Roman manufacture. The Teutonic population of this part of the island buried their dead entire, and cremation did not prevail among them. But the case was different in other parts

of the island. Somewhat more than twenty years ago, early cemeteries were accidentally discovered in some of the Midland counties, such as Warwickshire, Derbyshire, Nottingham, and Leicestershire, filled with cinerary urns presenting great novelty of character, but which were at first hastily set down as British, though a little consideration was sufficient to throw doubt on the correctness of this appropriation. I believe that Mr. Roach Smith and myself were the first to insist on the Anglo-Saxon character of this pottery, and to point out the evidence that the Angles in Britain usually burned their dead, while the Jutes and Saxons buried the bodies entire. Further discoveries, and a comparison of the various objects found in the urns and in close relationship with them, confirmed the opinion that these urns were purely Teutonic, and that we had thus a pottery presenting its own peculiar characteristics and belonging to our Teutonic forefathers. Our knowledge of this type of pottery was considerably extended by the extensive excavations in Cambridgeshire and Suffolk made by the late Lord Braybrooke, then the Hon. Mr. Neville, whose interesting museum at Audley End contains an extensive collection of the finest specimens of the East Anglian urn-ware. It is to these urns that antiquaries now chiefly refer as Anglo-Saxon pottery, and I have given in the accompanying plate six examples from the Audley End Museum, copied from Mr. Neville's handsome volume, entitled *Saxon Obsequies*, published in 1852.

The pottery is usually made of a rather dark clay, coloured outside brown or dark slate colour, which has sometimes a tint of green, and is sometimes black. These urns appear often to have been made with the hand, without the employment of the lathe; the texture of the clay is rather coarse, and they are rarely well baked. The more characteristic forms of the urns are represented in our plate, but they vary in some degree both in form and ornament. The favourite ornaments are bands of parallel lines encircling the vessel, or vertical, and zigzags, sometimes arranged in small bands, and sometimes on a larger scale covering half the elevation of the urn; and in this latter case the spaces are filled up with small circles and crosses, and other marks, stamped or painted in white. These circles, and some of the other ornamental marks, appear to have been impressed with the end of a stick, cut at right angles and notched. Other ornaments are met with, some of which are evidently unskilful attempts at imitating the well-known egg-and-tongue and other ornaments of the Roman Samian ware, which, from the specimens, and even fragments, found in their graves, appears to have been much admired and valued by the Anglo-Saxons. But a

still more characteristic peculiarity of the pottery of the Anglo-Saxon burial urns consists in raised knobs or bosses, arranged symmetrically round them, and sometimes forming a sort of ribs, as in Figs. 3 and 6 on our plate, while in the ruder examples they become mere round lumps, or even present only a slight swelling of the surface of the vessel.

That these vessels belong to the early Anglo-Saxon period is proved beyond any doubt by the various objects, such as arms, personal ornaments, etc., which are found with them, and they present evident imitations both of Roman forms and of Roman ornamentation. But one of these urns has been found accompanied with remarkable circumstances, which not only



NO. I.—ANGLO-SAXON POTTERY.

show its relative date, but illustrate a fact in the ethnological history of this early period. Among the Faussett collection of Anglo-Saxon antiquities, which now forms part of the large and valuable museum of Mr. Mayer, of Liverpool, there is an urn which Bryan Faussett appears to have obtained from North Elmham, in Norfolk, and which contained bones of a child. It is represented in the accompanying group of Anglo-Saxon pottery (No. I., Fig. 1), and will be seen at once to be perfectly identical in character with the East Anglian sepulchral urns which form our plate of Anglo-Saxon pottery. But Mr. Roach Smith, in examining the various objects in the Faussett col-

lection, preparatory to his edition of Bryan Faussett's *Inventorium Sepulchrale*, discovered on one side of this urn a Roman sepulchral inscription, which is easily read as follows:—

D. M.	To the gods of the shades.
LAELIAE	To Lælia
RVFINAE	Rufina.
VIXIT·A·XIII	She lived thirteen years,
M·III·D·VI.	three months, and six days.

To this Roman girl, with a purely Roman name, belonged no doubt the few bones which were found in the Anglo-Saxon burial urn when Bryan Faussett received it, and this circumstance illustrates several important as well as interesting questions relating to our early history. It proves, in the first place, what no judicious historian now doubts, that the Roman population remained in the island after the withdrawal of the Roman power, and mixed with the Anglo-Saxon conquerors; that they continued to retain for some time at least their old manners and language, and even their Paganism and their burial ceremonies; for this is the purely Roman form of sepulchral inscriptions; and that, with their own ceremonies, they buried in the common cemetery of the new Anglo-Saxon possessors of the land, for this urn was found in an Anglo-Saxon burial ground. This last circumstance had already been suspected by antiquaries, for traces of Roman interment in the well-known Roman leaden coffins had been found in the Anglo-Saxon cemetery at Ozingell, in the isle of Thanet; and other similar discoveries have, I believe, been made elsewhere. The fact of this Roman inscription on an Anglo-Saxon burial urn, found immediately in the district of the Anglo-Saxon cemeteries which have produced so many of these East Anglian urns, proves further that these urns belong to a period following immediately upon the close of what we call the Roman period.

The sepulchral vases found in what we may consider as the district of the middle Angles, which included the counties of Derby, Nottingham, and Leicester, vary but slightly from the East Anglian burial urns. An example of them is given in Fig. 2 of our group of Anglo-Saxon pottery, No. I. It was found at Chestersovers, in Warwickshire, and was accompanied with an iron sword, a spear head, and other articles of Anglo-Saxon character, of which there could be no doubt, or, indeed, of the other remains found in the same cemetery. It is right to remark that, while choosing the burial urns as the representative of Anglo-Saxon pottery, I do so because they give us the most extensive and therefore the surest means of comparison; but I do not mean this to imply that there are

Anglo-Saxon Pottery.

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not other characteristic sorts of Anglo-Saxon pottery, though the earthen vessels for domestic purposes are much more rarely met with. We find sometimes in their graves small cups and basins, the forms of which are evident imitations of Roman earthenware, jugs of more Teutonic character, with or without handles, in the former case the exact prototype of our mediæval and old English jug; and vessels of other descriptions. One of these is represented in Fig. 3 of our cut No. I. It is a vessel of pale red clay, between four and five inches high, and was found by the late Mr. Rolfe of Sandwich, in one of the Saxon graves opened by him at Ozingell, in Thanet. The original will be found in the museum of Mr. Mayer, who purchased Mr. Rolfe's collections.

If we had not abundant proofs of the Anglo-Saxon character of this pottery at home, we should find sufficient evidences of it among the remains of the kindred tribes on the Continent, the old Germans, or Alemanni, and the Franks. Some years ago an early cemetery, belonging to the Germans, or Alemanni, who then occupied the banks of the Upper Rhine, was discovered near a hamlet called Selzen, on the northern bank of that river, not far above Mayence, and the rather numerous objects found in it are, I believe, preserved in the Mayence Museum. They were communicated to the public by the brothers Lindenschmit, in a well illustrated volume published in 1848, under the title *Das Germanische Todtenlager bei Selzen in der Provinz Reinhessen.* When this book appeared in England, our antiquaries were astonished to find in the objects discovered in the Alemannic cemeteries of the country bordering on the Rhine a character entirely identical with that of their own Anglo-Saxon antiquities, by which the close affinity of the two races was strikingly illustrated. More recently, the subject has been further illustrated in the description by one of the Lindenschmits (Ludwig) of the collection of the national antiquities in the Ducal Museum of Hohenzollern, published in 1860,* and in several other publications. About the same time with the first labours of the Lindenschmits, a French antiquary, Dr. Rigollot, was calling attention in France to similar discoveries in the cemeteries which the Teutonic invaders of Picardy had left behind them, and in which he recognized the same character as that displayed by the similar remains of the Anglo-Saxons in our island. Similar discoveries have been made in Burgundy and in Switzerland, the ancient country of the Helvetii; and it is hardly necessary here to do more than mention the great and valuable researches carried on by the Abbé Cochet among the Frankish graves in Normandy, and so well de-

* *Die Vaterländischen Alterthümer der Fürstlich Hohenzoller'schen Sammlungen zu Sigmaringen.* 4to. Mainz, 1860.

scribed in his three successive volumes, which are, or ought to be, in the library of every English antiquary.* It has thus become an established fact that the varied remains of the tribes, all of Teutonic descent, who settled on the borders of the Roman empire along the whole extent of country from Great Britain to Switzerland, present the same character and bear a close resemblance.

A few figures will be sufficient to illustrate this resemblance as far as regards the pottery, and these are given in our group

NO. II.—FRANKISH AND ALEMANNIC POTTERY.

No. II., in which Figs. 4 and 5 are Alemannic vases from the cemetery of Selzen. It will be seen that they resemble exactly in form those East Anglican urns we have given in our plate, and the same ornamentation is also found among our Anglo-Saxon pottery. These urns are described as being usually made of the clay of the neighbourhood, in most cases turned on a lathe, but many of them imperfectly baked. They are found in graves where the body had not undergone cremation, and were used for containing articles of a miscellaneous description. In one

* *La Normandie Souterraine, ou Notices sur des Cimetières Romains et des Cimetières Francs explorés en Normandie*, par M. l'Abbé Cochet. Second edition. 8vo. Paris, 1855.—*Sépultures Gauloises, Romaines, Franques, et Normandes, faisant suite à "La Normandie Souterraine,"* par M. l'Abbé Cochet. 8vo. Paris, 1857.—*Le Tombeau de Childéric Ier Roi des Francs*, par M. l'Abbé Cochet. 8vo. Paris, 1859.

grave, at the feet of the skeleton of a gigantic warrior, was found one of these urns, containing two bronze fibulæ, a comb, a number of beads, a pair of shears, flints and steel, and a bronze ring. I mention this to show that these urns were not necessarily used only to contain the ashes of the dead, though they were used for this purpose by people among whom the practice of cremation prevailed. One vase found in a grave in the cemetery at Selzen exactly resembled the Anglo-Saxon vessel given in our first group, No. 3, of which, at least, one further example has been found in England. Fig. 1 is an urn procured by Mr. Roach Smith at Cologne, and engraved in his *Collectanea Antiqua*, vol. ii., plate xxxv.; it is now in the museum of Lord Londesborough at Grimstone Park in Yorkshire. It was stated to have been found in a grave with a skeleton and other objects, on the outside of the gate of St. Severinus, at Cologne. In form it resembles the urn, Fig. 6, on our plate, and is, like it, slate-coloured, with a similar ornament of circular stamps.

Figs. 2 and 3 of our group No. II. are Frankish urns obtained by the Abbé Cochet from his extensive excavations at Londinières in Normandy, and show at a glance the identity of the Frankish pottery with the Germanic as well as with the Anglo-Saxon. The first of these is surrounded with a row of the well-known bosses, which are equally characteristic of the three divisions of this Teutonic pottery, Anglo-Saxon, Frankish, and Alemannic. Above these bosses is an ornament identical with that of the East-Anglian urn with the sepulchral inscription, given in our first group, Fig. 1. The urn represented in Fig. 3 has an ornament which is evidently an imitation of the egg-and-tongue ornament so common on the Roman pottery.

The Abbé Cochet has collected in the course of his excavations in Normandy several hundreds of these Frankish urns, which all present the same general character. He states that the prevailing colour is black, produced, as is proved by chemical analysis, by a varnish of plumbago, which, either from being so long buried in the earth or from some other cause, is easily washed off, and which varies in shade from a very deep tint to almost gray. The pottery of the darker tints is usually the finest in texture and the richest in ornament, while the gray or nearly gray pottery is usually thicker, coarser, and plainer. The ornaments are almost always stamped, or incised, and consist, to use the Abbé's own words, of "zig-zags, St. Andrew's crosses, the teeth of saws, fern leaves, circles, plaited work, ovals, dotted work," and a variety "of other ornament well known from the Saxon and Carlovingian monuments." He states that they were usually placed at the feet of the skeletons, and that they are found either empty, or filled with the

earth which had fallen into them; and he suggests that they were placed in the graves filled with "lustral water," which was intended to protect the graves from evil spirits. This, too,

NO. III.—POTTERY FROM SWISS LACUSTRINE HABITATIONS.

he seems to think was only a secondary use of the vessels themselves, for he usually found them—those especially of light

colour—bearing unmistakeable traces of having been exposed to fire and smoke. From this circumstance the Abbé Cochet believes that these vases had served for ordinary domestic purposes before they were deposited in the grave along with the dead.

I have yet another group of pottery to introduce, which is given in our cut No. III. It consists of earthen vessels found in the lacustrine habitations of Switzerland, of which so much has been written during the last few years. I have taken them from the plates illustrative of the communications of Dr. Ferdinand Keller to the *Transactions* of the Antiquarian Society of Zurich. I have seen some of this pottery strangely misappropriated, and feel surprised that, as far as I know, nobody has pointed out its real character. The first of these examples, No. III., Fig. 1 (which is Fig. 23 of Plate ii. of the *Mittheilungen der Antiquarischen Gesellschaft in Zurich*, band xiv., heft 1), is a fine vase found in the *Pfahlbauten*, as these lacustrine habitations are called by the German antiquaries, at Sesto Calende on the Lago Maggiore, on the borders of Switzerland and Italy. There can be no doubt that this belongs to the same class of pottery which is the subject of the present paper—it is Germanic in form and character, and the identity of ornamentation will be more fully understood by comparing it with the fragments found also at Sesto Calende, and represented in Figs. 24, 25, and 26 of the same plate in the Zurich *Transactions*. Figs. 27 and 28 of that plate, found at the same place, are also undoubtedly Germanic; one is a variety of the jug-shaped vessel found in the Alemannic, Frankish, and Anglo-Saxon graves (see Roach Smith's *Collectanea Antiqua*, vol. ii., plate lii.), and the other is an equally well-known form of Anglo-Saxon, Frankish, and Germanic pottery. My examples, Figs. 2 and 3 (taken from the Zurich *Transactions*, band xiv., heft 6, plate i., figs. 9 and 12), found, as I understand it, on the Lago del Garda, on the borders of the Tyrol and Italy, although the antiquaries of the Pfahlbauten set them down as Celtic, present the unmistakable bosses of the Teutonic pottery; and if the reader will turn to Smith's *Collectanea Antiqua*, vol. ii., plate liii., he will find figures of the exact counterparts of them, even in their very rudeness of form, which were dug up from an Anglo-Saxon cemetery, excavated in 1844, at Kingston, near Derby. My last example, Fig. 4 (taken from the Zurich *Transactions*, band xiv., heft 6, plate viii., fig. 13), was taken from a Pfahlbau near Allensbach on the Untersee, on the borders of Switzerland and Germany, and appears also to be ascribed to a very remote period; it presents a form and ornaments recognized at once as belonging to the Alemannic, Frankish, and Anglo-Saxon pottery. I think it right to add

to the comparison of these two or three examples, that, in all the engravings of objects found in the lacustrine habitations I have yet seen, the pottery seems to me to be universally of the same character, a matter of very serious consideration for those who shall write in future on these remarkable Swiss remains. To the present time, their predecessors have been making appropriations which will not stand for a moment before the careful researches of the judicious antiquary. M. Troyon (*Habitations Lacustres*, plate vii., fig. 35) ascribes a vase to the "stone age," and (in plate xiii.) gives another to the so-called "age of bronze," both which belong undoubtedly—I might almost say ridiculously so—to the Alemannic or Frankish pottery! It is certainly a circumstance not to be overlooked, that over the whole extent of Switzerland to the forest borders of Italy and Germany, all the pottery found in these lacustrine habitations should belong to one class, and that belonging to a period embracing probably the greater part of the fifth and sixth centuries after Christ.

ROSS'S NEW $\frac{1}{12}$TH.—THE REQUISITES FOR HIGH POWERS.

THE letter of Mr. Brooke which we published in our last number will help to stimulate microscopists to pay attention to the qualities they should seek in deep objectives for the microscope, and to the reasons for selecting one power in preference to another. We hope Mr. Brooke will continue his inquiries, and, in the meanwhile, we beg to offer the following remarks.

The question raised by Mr. Brooke's letter is whether a $\frac{1}{12}$th objective, of large angular aperture and exquisite workmanship, is to be accepted as the glass of the shortest focal length which it is desirable for ordinary purposes to make or employ. The late Mr. Ross, after experimenting on the subject, was of opinion that a $\frac{1}{12}$th objective could be made so as to take in all the light *practically* obtainable from any object, and that it admitted of more perfect corrections than could be given to combinations of shorter focus. The small diameter of the front lens of high powers precludes their admitting the quantity of very oblique rays usually indicated in describing their angular aperture. The real quantity of such rays is considerably less than that computed according to formula in current use; but it is easy to understand that the front lens of Mr. Ross's $\frac{1}{12}$th, when brought within about 006″ of an object, may take in quite as much, or more, of the oblique rays than could enter a smaller

lens in closer proximity. Now, if you receive through a given glass all the ossilike light, and the corrections are extremely fine, it is obvious that you can raise the magnifying power by eye-pieces to the highest *practical* extent; that is to say, you can magnify your object as long as all the light it can emit with a given illumination enables you to see it. Of course, in pushing the power of a glass in this way it is exposed to the severest test; and Mr. Thomas Ross can desire no higher testimony to his skill than the fact that both Mr. Brooke and Mr. Lionel Beale have seen extremely minute and very delicate objects with the new $\frac{1}{25}$th in a manner that has not been surpassed by the performance of any other glass.

We are indebted to the courtesy of Mr. Ross for an opportunity of trying two of the new $\frac{1}{25}$ths upon a variety of objects and under different conditions, and we can emphatically endorse the very high praise they have received from other observers. They are managed with great facility, the definition is exquisitely sharp and clear, and the penetration large in proportion to the angular aperture they possess. What more can be desired? For a great many purposes we say nothing more can be desired, until new materials are at the disposal of the optician, or some novel and unexpected optical formula is devised.

Messrs. Powell and Lealand's $\frac{1}{25}$th is so beautifully corrected as to leave very little possibility for improvement in this respect; but Mr. Ross may still be right in the opinion that minute glasses in the front of any combination tend to introduce certain errors of diffraction, and that, in stopping at his $\frac{1}{25}$th, he is able to keep these errors down. When two great artists, such as Ross and Powell, both do their best, we cannot expect the balance of merit will be easy to discern; and it would require a prolonged and very elaborate series of experiments to determine whether everything that can be shown with the $\frac{1}{25}$th can also be shown with $\frac{1}{12}$th when raised to the same power. It is, however, certain, that if there be cases in which the $\frac{1}{25}$th would surpass its rival, they must be very few. The foremost obstacle to the use of the $\frac{1}{25}$th, and which does not affect the $\frac{1}{12}$th, is the closeness of its approximation to the object. It cannot be worked through glass that will bear handling, and consequently it is better adapted for the display of a carefully-prepared object, than for research under the usual difficulties which the examination of tissues, etc., entails.

In a former paper we praised Smith and Beck's $\frac{1}{25}$th as an instrument of research. Its form (that of a dialyte) probably does not admit of the *very finest* corrections that *are possible;* but it has an excellent definition, *great penetration,* works through glass of the thickness suited to Ross's $\frac{1}{12}$th, and from the mode-

rate proportion which its angle of aperture bears to its focal length, it gives good indications when a little out of adjustment. These are qualities of great value.

For exhibiting known and well-prepared objects, or for researches in which the finest attainable definition of what is seen is preferable to seeing a little more but not quite so well, the new $\frac{1}{12}$th of Mr. Ross will realize all that the most exacting microscopist will desire, and no one will make trial of this glass without wishing to have it. But there remains a large class of inquiries for which we think Mr. Ross could provide better than by his $\frac{1}{12}$th. If he constructed a glass of somewhat shorter focus, and of smaller angular aperture, it would, in all probability, be a less perfect specimen of optical skill than the $\frac{1}{12}$th, and decidedly inferior to it for the purposes we have mentioned; but we think it would excel it in certain common cases. It would, we will suppose, be infinitesimally inferior in definition; but it would have a noticeable increase of penetration, and be less affected if not in the best possible adjustment for the object in view. Mr. Ross has probably done as much as is possible in the way of reconciling the opposite demands for large angular aperture and considerable penetration; but it is, in our opinion, often advisable to have recourse to a glass in which the penetration is increased, and the angle of aperture diminished. When you know exactly what to look for in an object, it is very easy to correct such a glass as the new $\frac{1}{12}$th for the thickness of covering, or the density of a layer of fluid through which the object is seen; but in viewing the interior of minute living objects, or minute portions of organs that will not bear much compression, it is by no means easy to regulate a very delicate glass to the exact state of adjustment which is best adapted to the occasion; and, in such cases, a small quantity of other good qualities may be advantageously sacrificed to secure a little more penetration. We do not see how any one glass of deep power can satisfy the two sets of demands upon them. While, therefore, we accord the highest place to Mr. Ross's new $\frac{1}{12}$th, and freely admit the wide range of purposes for which, perhaps, nothing else can be so good, we think there is ample room for the employment of an obective differing from it in the manner we have explained.

COMETS.

AN ACCOUNT OF ALL THE COMETS WHOSE ORBITS HAVE NOT BEEN CALCULATED.

BY G. F. CHAMBERS.

(*Continued from page* 376, *vol.* v.)

904. At about the time of the birth of the Emperor Constantine Porphyrogenetus, a brilliant comet showed its rays in the E. It lasted forty days and forty nights.—(Leo Grammaticus, *Chronographia*, p. 483.) Constantine was baptized on the Festival of the Epiphany, or on Jan. 6, 905, so the comet may be dated for Nov. and Dec. 904.

905. On May 22 a comet was seen near α and β Geminorum. Its tail was 30° long and reached to the fore-feet of the Great Bear. On June 12 the comet went out from α and γ Leonis; on June 13 clouds obscured the sky, and on June 18 the comet had disappeared.—(Ma-tuoan-lin.) From the European account in the *Chronicon Floriacense*, it would rather seem that it was the *head* of the comet which was in Ursa Major, and that the tail reached to the zodiacal region; but the description is altogether very vague, and the Chinese testimony generally is preferable in such cases.

912. A comet appeared for fifteen days in the W. like unto a sword.—(Leo Grammaticus, p. 487.) It lasted for fourteen days in the N.W. in March.—(Hugo, Monachus Floriacensis, *Chronicon.*) On May 13 a comet went out from the sidereal division of ν Hydræ to near χ Leonis.—(Ma-tuoan-lin.) Probably *Halley's Comet*, the P. P. occurring early in April.—(Hind.)

912 or 913. A comet was seen in Egypt in the year 300 of the Hegira.—(Hali, *Comment. in Ptolemæus.*) This year commenced on August 18, 912, and ended on August 6, 913.

923. In October or November a comet was seen near γ and δ Cancri.—(De Mailla, vii. 210.)

928. On December 13 a comet came from the S.W. Its R.A. was 5° greater than that of β Capricorni. Its tail was 10° long and pointed to the S.E. After three evenings it ceased to be visible.—(Ma-tuoan-lin.)

936. On September 21 a comet came from the sidereal division of β and α Aquarii. It was 1° long and passed near ξ Aquarii and ρ Capricorni.—(Ma-tuoan-lin.)

939. "There was seen in Italy for eight successive nights a comet of surprising grandeur; it threw out rays of extraordinary length."—(Luitprandi, Ticinensis, *Rerum Gestarum*, v. 1.) Possibly July was the month.

941. On September 18 (or perhaps on November 17; it is not possible to say which) a comet went out from the W.; it swept the walls of *Tien-ché*, and was 10° long.—(Ma-tuoan-lin.) It was seen in October for three weeks.—(*Chronicon S. Florentii.*)

942. In October a comet appeared for three weeks in the W. It had a long tail, and advanced gradually eastwards to the meridian.—(*Chronicon Andegavense.*) Several authorities say that the comet appeared only for two weeks, from October 18 to November 1.—(Witichindi. *Annales*, etc.) All remark that a great mortality amongst oxen occurred in the following year *in consequence* [?]

943. On November 5 a comet appeared in the E. country. Its R.A. was greater than that of *a* Virginis by 9°. Its tail was 1° long and pointed to the W.—(Ma-tuoan-lin.) Comets were seen for fourteen nights.—(*Annalista Saxo.*)

945. Theotilon, Bishop of Tours, set out from Laon to return to his diocese, but was overtaken on the road by the malady of which he died. He had just partaken of the holy sacrament when a luminous sign was seen traversing the sky. This sign was a cubit long. Its brilliancy was such that it gave light in the middle of the night to those who were charged to conduct to Tours the body of the prelate, by a journey of 200 miles."—(Frodoard *Chronicon.*) Pingré considers that, apart from other testimony, the duration determines this to have been "une veritable comète."—(*Hist.* i. 356.)

956. On March 13 a comet was seen in the Cross of Orion. It tail pointed to the S.W.—(Ma-tuoan-lin.)

959. At the time of the death of the Emperor Constantine Porphyrogenetus, a gloomy and obscure star appeared for some time.—(Constant. Porph. *Incerti Continuatoris*, p. 289.) Constantine died on November 9. It was seen from October 17 to November 1.—(Tackius.)

975. A bearded comet was visible from August to October.—(Cedrenus *Compendium Historiarum*, p. 683.) It was first seen on August 3 in the head of Hydra, between seven and nine hours of the morning; the tail was 40° long. The comet traversed Cancer, and came to the square of Pegasus, and lasted altogether twelve weeks.—(Gaubil.) It became visible on the 5th moon, which terminated on July 11.—(De Mailla, viii. 58.) There is much reason to believe that this comet is identical with the celebrated ones of 1264 and 1556. Presuming the P. P. to have taken place at the end of July, the above accounts will all harmonize extremely well.—(Pingré, i. 357.)

981. A comet appeared in the Autumn.—(Burkhardus, Monachus S. Galli, *Historia*, i.)

985. A comet appeared during the Pontificate of John XVI. —(Platinæ, *De vitis summorum Pontificum.*)

989. [i.] On February 10 a comet appeared to the N. of *a* and *β* Pegasi. It was 1° long and lasted fourteen days.—(Gaubil; *Annalista Saxo.*) Pingré questions the authenticity of Gaubil's citation.—(*Comèt.* i. 620.)

990. A star, with a long tail, appeared in the N.; after some days it was in the W., and its tail extended to the E.—(Romualdus Salernitanus, *Chronicon.*) It was seen in August or September in the W.—(Couplet.)

995. On August 10 a comet was seen.—(Hepidannus *Annales;* Florentius Vigorniensis, *Chronicon.*)

998. In February a comet was seen to the N. of *a* and *β* Pegasi.—(Couplet; De Mailla, viii. 131.)

1000. A comet appeared on December 14 for nine days. It frightened everybody. — (Iperius *Chronicon* xxxiii.) A meteor appeared at the same time, and the majority of the writers confound the one with the other.

1003. [i.] In the month of February a comet was seen, it disappeared near the sun, and was only seen for a few days a little before the rising of that body.—(Hepidannus *Annales.*)

1003. [ii.] A comet appeared during the Pontificate of John XVII.—(*Chronicon Nurembergense.*) It lasted a long time.—(*Chronicon Stederburgense.*) It was discovered in China on December 23; it approached very near *θ*, *τ*, *ι*, *υ*, *φ* Geminorum, passed by *a* and *β* Aurigæ, *β* Tauri, to the Cross of Orion, and disappeared after thirty days. Its tail was 4° long and like a vase in shape.—(Ma-tuoan-lin.) Some European authors refer to a comet in 1004, which is probably this one prolonged. Pope John was elected on June 13, and lived only till December 7, so can there have been two comets between June, 1003, and December—January, 1003–4?

1005. A comet was seen in the S. — (Alpertus, *De diversitate temporum.*) It was in the W. in September, at the commencement of night, and lasted three months. It shone with great brilliancy and did not set till cock-crowing.—(Glaber Rodolphus, *Annales.*) It was seen in China within the circle of perpetual apparition.—(De Mailla, viii. 158.)

1012. A comet of extraordinary grandeur was seen for three months in the Southern part of the heavens.—(Hepidannus.)

1015. A comet was seen in February.—(Protospat, *Chronicon.*)

1017. A comet, like a large beam, was seen for four months. —(Sigebertus, *Chronographia;* Gerbrandus, *Chonicon Belgicum,* ix. 7.)

1018. On August 4 a comet appeared to the N.E. of (appa-

rently) ζ Ursæ Majoris, it was 3° long and went Northwards. It passed by ω Ursæ Majoris, and thence Southward (Ma-tuoan-lin), by a route which Pingré says must have been erroneously stated. However, it is certain that a comet appeared this year in the Polar regions, and that it lasted about six weeks.—(Ditmarus, *Chronicon*, viii.) It is less certain that its length increased to 30°, and that passing Leo it disappeared in Hydra.

1023. A comet appeared in Leo during the autumn.—(Ademarus, *Chronicon*.) The original account contains much that is certainly fictitious.

1024. A comet appeared the year before the death of Bolestas I. King of Poland.—(Dlugossus, *Historia Polonica*.)

1033. A comet 2° long, appeared on March 5 to the "East of the north country" [N.E.?].—(Ma-tuoan-lin.) It appeared on March 9 about the tenth hour of the night, and lasted for three nights till sunrise.—(*Fragmentum Historiæ Francorum*, i. and ii.)

1034. A column of fire was seen in the E. in September.—(Cedrenus, p. 737.) It appeared between κ and φ Hydræ and Crater.—(De Mailla, viii. 199.)

1035. [i.] A comet appeared in κ and φ Hydræ and Crater. It was 7° and 5 tsun long.—(Ma-tuoan-lin.) Possibly this is identical with the preceding. If 1035 is the right year then the column of fire probably was a meteor.

1035. [ii.] On November 11 a comet, with a faint tail, appeared in the western ribbon of Pisces.—(Ma-tuoan-lin.)

1041. Comets appeared.—(Glycas, *Annales*, p. 316.)

1042. On October 6 a comet appeared. Its motion was from E. to W. and it lasted throughout the month.—(Glycas, p. 319.)

1046. A comet appeared in the fifteenth year of Henry I. of France.—(Godellus, *Chronica*.)

1049. On the morning of March 10, before sunrise, a comet was seen near β Aquarii, and α Equulei; it passed by the head of Orion, Musca, and the horns of Aries, and lasted sixteen weeks.—(Gaubil.) "La route qu'on assigne à cette comète n'est pas naturelle."—(Pingré i. 372.) Ma-tuoan-lin's route is scarcely more intelligible.

1056. In July or August a comet appeared in the circumpolar regions.—(De Mailla, viii. 245.) It seems to have passed southward to Hydra, but Gaubil places it in the head of Orion when first seen. Ma-tuoan-lin agrees with De Mailla. It was 10° long, and on September 25 had disappeared. [N.B. The head of Orion is *Tsoui*, the other region, *Tsé-ouey*; pronunciation nearly identical, hence possibly a confusion.]

1058. "The death of Casimir, King of Poland, was an-

nounced by a comet which appeared for several nights."—(Hennenfeld, *Annales Silesiæ.*) It lasted the whole of Easter week.—(Morigiæ, *Chronicon*, i.)

1060. A little after the death of Henry, King of France [August 29], a comet, with a long tail, appeared in the morning.—(Will. Malmsbury.)

1067. A comet appeared at the death of Constantine Ducas [May].—(*Chronicon Andegavense*, ii.)

1071—8. During the reign of Michael Parapinatius comets frequently appeared.—(Curopalatas *Excepta e Breviario historico*, p. 856.)

1075. On November 17, a comet 3° long, appeared in the middle of Corvus; on the 18th the tail was bifid and curved; and on the 19th its length was 5°; on the 20th its length was 7°, and it pointed towards η Corvi; on November 29 the comet entered the Hyades and disappeared.—(Ma-tuoan-lin; De Mailla, viii. 285.)

1080. [i.] On August 10 a comet 10° long appeared to the S. of Coma Berenicis; it was curved, and pointed to the S.E. Its R.A. exceeded that of γ Corvi by 8 or 9 degrees. On August 13 it reached towards the W. of the north country. [Pingré does not understand what is meant.] Its R.A. exceeded that of α Corvi by 9°; on August 15 it was 3° long, curved, and penetrated Coma Berenicis. On August [18 or 20] the comet passed very near α and γ Leonis. On August 24 the comet entered the Hyades and disappeared.—(Ma-tuoan-lin.)

1080. [ii.] On August 27 a comet (which Ma-tuoan-lin regards as the preceding again visible) appeared between κ and ϕ Hydræ and lasted till September 14. Pingré is our authority for separating these accounts.—(*Comèt*, i. 625.)

1096. On October 7, a comet like a sword appeared in the southern part of the heavens. — (Ekkéardus, *Annalista Saxo.*)

1098. On June 3, "the night of the capture of Antioch," a comet shone out with great brilliancy.—(Robertus, *Historia Hierosolymitana*, v.)

1101. On January 31, a large comet appeared in the W. soon after sunset.—(*Synopsis Chronologica.*)

1106. A splendid comet appeared this year; it was first seen on February 4, within 1½ feet of the sun, between the third and ninth hour of the day. In Palestine it became visible on February 7, and in China three days later. On February 7, it was at the beginning of the sign Pisces, and passed by the Northern Fish and southern arm of Andromeda, α and β Arietis, and the Pleiades to the Hyades. The comet remained visible for seven or eight weeks, and had a tail 63° long.—

(Matthew Paris, *Hist. Major*; Gaubil, Ma-tuoan-lin, and many others.)

1109. In December, a comet appeared near the Milky Way with a tail pointing towards the S.—(Hemingfort, *Chronica*, i. 33.)

1110. On May 29 a comet with a tail 6° long was seen near the Northern Fish and the horns of Aries. It went N. towards the Pole, and then became visible throughout the night, and ultimately disappeared in the R.A. of about 4h.—(*Chronica Regia S. Pantaleonis*; Ma-tuoan-lin.)

1113. A great comet appeared in May.—(Matthew Paris; Matthew Westminster, *Flores Historiarum*.)

1114. A comet at the end of May; it lasted several nights, and had a long tail.—(Henry of Huntingdon, *Historia*; *Annales Waverleienses*.)

1115. An extraordinary star in April—May, near α and β Leonis; it had a long tail, and probably was a comet, though no movement is referred to.—(De Mailla, viii. 377; *Annales de Margan*.)

1125. A comet preceded the death of Uladislas, King of Bohemia.—(Dubravius, *Historia Bojemica*, xi.)

1126 [i.] In June—July a large comet was seen within the circle of perpetual apparition; it passed from α Herculis towards θ and ϕ Ursæ Majoris.—(De Mailla, viii. 443.) These Chinese positions will not harmonize with the statements of the Latin historians, unless we suppose the comet to have been in Ursa Major at the end of July, or rather at the beginning of August.—(Pingré, i. 392.)

1126 [ii.] In the moon beginning on December 15 a great comet was seen in China near the horizon.—(De Mailla, viii. 447; Ma-tuoan-lin.)

1132 [i.] On January 5 a comet was seen.—(Ma-tuoan-lin.)

1132 [ii.] On October 2 a comet appeared; on October 7 it was in Musca; on October 27 it had disappeared.—(Florentius Vigorniensis, *Chronicon contin.*; Ma-tuoan-lin.)

1138. In August—September a comet appeared.—(De Mailla, viii. 524.)

1142-3. In December—January a comet appeared.—(*Synop. Chronol.*)

1145. On April 15 a comet appeared.—(*Calendarius Ambrosianæ Bibliothecæ*.) It is not easy to reconcile the conflicting accounts of its course through the heavens. In China it was first seen in the E. on April 24; on May 14 it was in the Cross of Orion [and must have had a considerable N. latitude, or would not have been visible.—Pingré], and had a tail pointing to N.E. 10° long.; on June 4 it was like a star; on June 9 it was stationary between α Hydræ and Crater, and remained

visible till July 14.—(De Mailla, viii. 545.) On April 26 a comet came from the constellations of the E. country. [These are probably the seven first of the Chinese Zodiac, commencing at α Virginis.—P.] After fifty days it disappeared; on July 13 it reappeared in the Cross of Orion, and lasted fifteen days.—(Ma-tuoan-lin), who adds that a comet was seen on June 4 (when the above one was still visible). Hind considers the former to be certainly *Halley's Comet*, and that it passed P.P. on April 29.

1146. A comet was seen for a long time in the W.—(*Chronica Regia.*)

1147 [i.] The Emperor Conrad set out in May for Palestine; his departure was preceded by a comet.—(*Historia Episcoporum Virdunensium.*) On February 8 a comet 10° long. appeared in the E. for fifteen days.—(Gaubil.) On January 11 a comet came from the S.W. of α Aquarii and ϵ, θ Pegasi.—(Ma-tuoan-lin.) This writer says that another comet appeared to the E. of the N. region, near σ and β Capricorni, on February 17, and that it perished on March 5.

1147 [ii.] About August 20 a comet was seen in Japan.—(Kaempfer, ii. 4.)

1152 or 1156. Ma-tuoan-lin the former, Gaubil and the great annals of China the latter. On July 26 a comet 10° long was seen in the feet of Gemini; on the day *Kouey-tchéou*, or August 2, it was near θ Geminorum.—(Gaubil.) On August 15 a comet was seen in the middle of Gemini; the next day it was like Jupiter, and 2° long. On the day *Kouey-tchéou*, or August 22, a comet passed near θ, τ Geminorum.—(Ma-tuoan-lin.) [No doubt the former date of August 2 is a misprint by Pingré for August 22.]

LITERARY NOTICES.

METAMORPHOSES OF MAN AND OF THE LOWER ANIMALS. BY A. DE QUATREFAGES, Membre de l'Institute (Academie des Sciences), Professeur du Muséum de l'Histoire Naturelle de Paris. Translated by Henry Lawson, M.D., Professor of Physiology in Queen's College, Birmingham, one of the Lecturers on Natural Science in the Science and Art Department of the Committee of Council on Education, etc. (Robert Hardwicke, 192, Piccadilly.)—In the second volume of the INTELLECTUAL OBSERVER, p. 95, will be found an article entitled "The Origin and Transformation of Animals," which was founded on the work of M. Quatrefages, which has lately enjoyed the benefit of Professor Lawson's translation. We must refer our readers to the article just mentioned for an exposition of the character of this important book, which stands alone in its

class, there being, as we believe, no other in which the recent philosophy of the transformations which living beings undergo from the egg to their complete development, is so fully explained. This work of M. Quatrefages is very superior to his *Rambles of a Naturalist*, which was too egotistic and too diffuse, and we are very glad that it has fallen into the hands of a translator of sufficient attainments to do justice to its great merits. M. Quatrefages' views on that method of reproduction which Professor Owen calls "Parthenogenesis" are much in accordance with those put forward by Dr. Carpenter, and the various facts and considerations which he adduces will be generally considered fatal to the Owen hypothesis. He does not, however, deny that certain cases occur to which Mr. Owen's term may be fairly applied. He says, "notwithstanding my reservations, parthenogenesis is still to my mind a constant phenomenon. With my *confrères*, I believe that there exist true females which deposit genuine ova, that are developed without any intervention on the part of the male. But I believe this phenomenon is far less frequent than has been supposed." M. Quatrefages wisely abstains from dogmatizing on this curious question, but he remarks that among certain animals the infecundated ovum has been shown to exhibit a series of movements "quite analogous to those which in the fecund ovum correspond to the formation of a new being," and he conceives that if in certain cases the vital force of the ovum is intensified, the interposition of the male may be dispensed with, and the incidents of the case may still be associated with the ordinary phenomena of generation. Broadly considered there will remain two modes of reproduction—one by buds, through which whole generations may arise as the consequence of an original creature having sprung from a true egg fecundated by a male—and another by a recurrence to the co-operation of a male and female parent giving birth to a fresh individual, from which new generations may again spring through the budding process. M. Quatrefages deserves great credit for exhibiting a most important mass of vital phenomena in their true scientific co-ordination. He has given us the results of his researches in a form that possesses a high degree of literary merit, and Dr. Lawson's translation, which we have carefully compared with the original, is justly described in a published letter of M. Quatrefages as *aussi élégante que fidèle*.

Sight and Touch; an Attempt to Disprove the Received (or Berkleian) Theory of Vision. By Thomas K. Abbott, M.A., Fellow and Tutor of Trinity College, Dublin. (Longmans.)—This is an elaborate attack upon the received theories of vision, according to which the eye is incapable of judging of size, distance, and real figure, until touch and other senses have been called into play, and have afforded a set of associations which may be made use of in interpreting visual impressions. Mr. Abbott contends that in "the eye, and the eye alone, there is a determinate sensation or state of the organ corresponding to a determinate distance of the object;" and that thus distance is directly perceived. We may not exactly apprehend the sense in which this statement is made, as in some places the writer appears to affirm his proposition absolutely,

and in others, with limitations that bring it more in accordance with the opinions usually held. Mr. Abbott says, "when we look from a near object to one a little farther off, the ocular adjustment is accompanied with a certain motion more suitable indeed than that of the hand to suggest distance; because for the same distance it is always the same; but still only a succession. But the former object continues to be seen, although with decreasing distinctness. Through a certain short interval both objects may be seen distinctly. This co-existence demands the idea of space as the indispensable form of its intuition. But what space? Not the superficial extension already known, the perception of which is accompanied with a totally distinct sensation. Suppose, for example, such a figure as a cube or the letter L placed on the table so that the horizontal line is directed towards us. In looking along the vertical line we are aware of the co-existence of certain distinctly visible parts, and by a certain motion can bring each in succession into the axis of vision. Thus we pass by a continuous change to the foot of the vertical. Now it will be proved presently that the eye requires a varying adjustment, in order to see distinctly at different distances. In moving the eye therefore along the horizontal line we find a new kind of sensation added to the former, and we also find the previously seen parts continue to be seen, but with a quite different kind of distinctness from before. If the horizontal line be near and long enough the parts not directly looked at will be seen double; and this will furnish another peculiar antecedent. Here then first are the conditions for the perception of trinal extension." The description given here is not that of seeing distance, but that of seeing different things, or different portions of the same thing in linear succession. Mr. Abbott appears to us mistaken in the extent of his denial, that we have to learn to see, and do so with the help of touch and locomotion. Many distinguished reasoners on this subject have not adequately considered the various motions and adjustments of which the eye is susceptible, and with which it can perform optical experiments, and thus obtain a series of sensations indicative of extension in three directions, or of distance. Some passages which Mr. Abbott quotes and objects to, are likewise not sufficiently explicit, as where Mr. J. S. Mill states, "The information obtained through the eye consists of two things—sensations, and inferences from those sensations; that the sensations are merely colours variously arranged, and changes of colour." We presume Mr. Mill, though incorrectly, includes simple light and shade under the term colour, but the "colours variously arranged," are arranged on the retina in definite forms, so that in certain positions of visual objects, the apparent form and the real one will coincide. Again, when Mr. Mill observes, "we judge an object to be some distance from us by the diminution of its apparent magnitude, that is by linear perspective, or by that dimness and faintness of colour and outline which generally increases with the distance, in other words, by aerial perspective;" he omits to notice the adjustments of the eye for binocular vision, and those required to bring divergent, or

parallel rays to a focus. In animals it would appear that to a large extent the interpretation of visual phenomena is *instinctive*—whatever that term may mean. They certainly do not go through the course of practical education that seems necessary for a man to learn to see. Mr. Abbott claims, if we understand him rightly, a large share of this same instinct for the human race, and he considers that the behaviour of children, and of persons who have received sight as the result of a surgical operation, confirms his view. He cites on his side Schroeder van der Kolk, who says, "At a very early period, the infant can bring his little hands to his mouth; subsequently in the third month he catches at an object to endeavour to raise it to himself. Proper touching and handling succeed much later, and demand a higher degree of mental activity and special investigation. Hence the absurdity of some writers that the child receives his first impression of distance and size by touch, and by feeling learns to see. On the contrary, he sees and distinguishes objects at various distances long before he seizes them in his hands and begins to examine them." It is certain that men are more influenced by instinct than is commonly supposed; but although the child may have some appreciation of distance before he has investigated relative distances, through touch and locomotion, it does not follow that he may not with their aid learn to see much better as he grows up.

Mr. Abbott is not disposed to allow sufficient importance to brightness and distinctness as an indication of distance. Apart from the knowledge of how bright and how distinct objects usually appear at particular distances, such indications would be worthless, and they are fallacious under new conditions; but when interpreted in conformity with sufficient experience they are solid guides.

Philosophers have usually ridiculed the popular notions that the moon really looks a foot wide, or that Orion seems to have a belt a yard long; but Mr. Abbott refers to the curious fact that "a pencil has the same apparent magnitude at four inches as at forty;" and he states that at great distances, objects such as we have mentioned have in all ages produced about the same impression of their apparent size on ordinary eyes.

Some of Mr. Abbott's statements appear to have been hastily made, as when he objects to the distinctness of an object affording an indication of its distance, he says, if so, "everything out of focus would seem very distant." From a distant object the eye receives only parallel rays, and when they are well focussed the eye is not annoyed as it is by looking at an object in which the focussing is bad. We have found his book very interesting and suggestive, and although our readers will differ from many portions, they will thank him for raising a discussion that must tend to the elimination of truth.

PHYSICAL GEOGRAPHY FOR SCHOOLS AND GENERAL READERS. By M. F. MAURY, LL.D., Commander in the Navy of the Confederate States of America, author of the "Physical Geography of the Sea," "The Wind and Weather Charts," "Maury's Sailing Directions," etc. (Longmans.) This little work affords popular explanations of many

facts pertaining to its subject; but it has not been put together with sufficient care. In many places the style is awkward, as where Mr. Maury tells us "every river runs in a valley, as the Valley of the Ganges, the Valley of the Nile, of the Rhine, of the Amazon—which means all the country that is drained by those rivers." In explaining what a hill means in physical geography, Mr. Maury will not help many students by telling them "ants and moles make hills." The coral polyp is called an *insect*, and the absurd statement made that "each one is *impaled* in his own little workshop." Further on we find an assertion in defiance of the well-known mathematical demonstrations of Hopkins and Thompson, "that at a depth of twenty-five or thirty miles below the spot on which you are now standing, everything, even the rocks, is in an incandescent and molten state." Notwithstanding the author's reputation, we cannot recommend a work in which such clumsiness and such blunders appear.

Advanced Text-Book of Physical Geography, by David Page, F.R.S.E., F.G.S. (William Blackwood and Sons).—Like other text-books of Mr. Page's, this is a well-selected and well-compressed compilation of all the leading facts relating to its subject. As a school-book, it would have the advantage of being decidedly interesting to the learner, and would afford an excellent basis for the higher kind of instruction. Geography should never be taught without due reference to physical considerations. As a mere catalogue of names, places and positions, it only wearies the mind, but, as Mr. Page has presented it, it becomes what it ought to be, an intellectual and entertaining pursuit. We should recommend in another edition a modification of the statements about the crust of the earth being a thin film, and all below it in a molten state, and of the passage ascribing volcanic action to the interior molten matter. The former statement is inconsistent with the fact that a thin crust could not resist the tidal action of a gigantic molten sea, and the researches of Mr. Mallet have shown that whatever may be the condition of the globe at great depths, earthquake phenomena arise from causes near the surface. It is also unadvisable to say that at a height of forty-five or fifty miles the earth's atmosphere is "inappreciable," as Quetelet has shown reasons for supposing that it is appreciable at much greater elevations. On the whole, we think very highly of Mr. Page's labours, and we should add that the present work is illustrated by numerous diagrams and maps.

Houses without Hands, by the Rev. J. G. Wood, M.A., F.L.S., with very numerous illustrations engraved on wood by G. Pearson, from original drawings made by F. W. Keyl and E. A. Smith, under the author's superintendence, expressly for this work. (Longmans.) —The interest of this popular work is well sustained in the recent numbers. The style is pleasing and the engravings excellent.

The Salmon. By Alexander Russell. (Edinburgh, Edmonstone and Douglas.) This work, part of which has appeared in periodicals, supplies a great deal of information on all subjects connected with the breeding of salmon, condition of rivers, legislature, etc.

WAYSIDE WEEDS; OR, BOTANICAL LESSONS FROM THE LANES AND THE HEDGEROWS, with a Chapter on Classification. By SPENCER THOMSON, M.D., L.R.C.S. Edin., Fellow of the Botanical Society of Edinburgh, Author of the "Structure and Functions of the Eye," etc., etc. Illustrated with engravings on wood. (Groombridge and Sons.) We could not recommend a pleasanter way of learning a great deal of elementary botany, than by making Dr. Spencer Thomson's *Wayside Weeds* the companion of country walks. A large number of plants easily obtained in any part of the country are described in a manner that will enable beginners to get over the initial difficulties of botanical science in a very agreeable manner. It is just the kind of book that families should take advantage of for recreative self-culture, and it might be advantageously introduced in the better class of schools. You can scarcely open a page without coming upon an elegant illustration, and the explanations show that Dr. Thomson understands the kind of help that young students require.

A SERIES OF METRIC TABLES, in which the British Standard Measures and Weights are compared with the Metric System at present in use on the Continent. By CHARLES HUTTON DARLING, Civil Engineer. (Lockwood and Co.)—There are few measures of practical reform that would effect more for the comfort and general interests of the people than the substitution of the so-called "Metric System" for our present abominably troublesome and unintelligible arrangements of weights and measures. Scientific men have long used the metric system for many of their operations, and in addition to its decimal character, it has the great advantage of logical coherence, so that if its principle is known, and the meaning of a small number of terms acquired, all its weights and all its measures are easily remembered and understood. When we consider how few Englishmen remember one quarter of the vexatious "tables" that constitute one of the chief torments of schoolboy days, and also bear in mind what an insufferable plague a bungling method introduces into what ought to be elementary and easy calculations, we cannot avoid hoping that the legislative permission to use the metrical system will not be thrown away. For those who wish to avail themselves of the metrical system in this country, Mr. Darling's book will afford admirable aid, and his well-arranged volume of tables will be indispensable in commercial houses engaged in Continental transactions.

THE PHYSICAL GEOLOGY AND GEOGRAPHY OF GREAT BRITAIN: Six Lectures to Working Men, delivered in the Royal School of Mines in 1863. By A. C. RAMSAY, F.R.S., Local Director of the Geological Survey of Great Britain. Second Edition. (Edward Stanford.)—We cordially welcome a second and improved edition of these admirable lectures. The new matter and improvements are considerable, and Professor Ramsay has given by way of frontispiece an extremely good geological map of Great Britain, which, though necessarily small in scale, is full of matter and very distinct. The great merit of these lectures is that they familiarize the reader

with the methods of reasoning and with much of the philosophy of geology, as well as make him acquainted with a highly interesting and well selected series of facts.

THE TEMPLE ANECDOTES. By RALPH AND CHANDOS TEMPLE. INVENTION AND DISCOVERY. Illustrated. (Groombridge and Sons.) —The rapid and remarkable success of this excellent publication show that its editors and publishers know what the public require. The numbers before us (2 and 3) fully equal the first one, and the engravings keep up the artistic character of the work. In No. 2 the incidents chosen by the artist are called "Parsley Peel," and "The Origin of our Cast Iron." In the first we see the founder of the Peel family engaged, as the story tells, in his kitchen devising a pattern for cotton printing, and availing himself of a suggestion made by his little daughter, that a sprig of parsley should form the text of the design. The pattern became famous and originated the familiar nickname of Parsley Peel. The second wood engraving illustrates the story told in Percy's Metallurgy of the Welsh shepherd boy, John Thomas, showing Abraham Darby the method he had discovered of casting an iron pot. This simple incident laid the foundation of an enormous manufacture. No. 3 gives us a picture of Jenner engaged with his discovery of vaccination, and a view of a steamboat passing down the Thames in 1814, when the new method of travelling first took possession of the river. The anecdotes in these numbers are well selected and well told; they will prove alike interesting to juveniles and adults.

PROCEEDINGS OF LEARNED SOCIETIES.

BY W. B. TEGETMEIER.

LINNEAN SOCIETY.—*June* 17.

DISCOVERY OF THE BONES OF A RECENT MOA.—Mr. Thomas Alldis exhibited to the society a number of bones, constituting nearly the entire skeleton of a Moa; these bones were in a very recent condition, the cartilages of the joints were not decayed, and many of the tendons and ligaments were in a perfectly fresh and flexible condition. The skeleton was found by some gold diggers under conditions which were not accurately described. The locality was somewhere near Dunedin, in the Middle Island of New Zealand. The bird had been destroyed whilst sitting on the nest, and the bones of the young ones which had perished with it, were found under those of the parent, the whole being imbedded in a deposit of shifting sand. Dr. Hooker suggested that the preservation of the soft tissues were due to the animal having been preserved in ice, as in the well-known case of the Siberian Mammoth, in which even the skin and flesh was perfectly preserved. But as there was no evidence of the existence of ice in New Zealand at the present time, it was suggested by Professor Huxley and others, that the animal must have been recent, and had probably not been dead more than ten or twelve years.

Should this opinion be correct, there is no reason why these gigantic birds (in comparison with which the ostrich itself is a pigmy) should not be living at the present time; such an event is not at all impossible. The forests in the interior of New Zealand have never been explored, even by the natives themselves, and the birds of the ostrich tribe are remarkable for their shyness and wariness. The ostrich itself in the open desert is only to be observed at a great distance, and a shy bird capable of quick movement on the surface of the earth might easily escape observation in the sparsely populated and dense forest of New Zealand. The bones are deposited in the museum of the Yorkshire Philosophical Society. In all probability they will prove on examination to be those of a male, as the females of this group of birds leave the duties of incubation to their mates. This fact has been noticed in those that have hatched in the Zoological Gardens and elsewhere. Thus the male Emu of Mr. Bennett at Brockham, hatched ten young birds this season, after setting fifty-nine days. The male Mooruk, *Casuarius Bennettii*, sits for seven weeks. The Rheas for five weeks, and the male Ostrich, which hatched in the Zoological Gardens at Marseilles in 1861, sat for forty-five days; the female taking no part whatever in the duties of incubation.

NOTES AND MEMORANDA.

Sun-spots and Planetary Positions.—Mr. Balfour Stewart continues the interesting inquiry which was explained in his important paper in No. xix. Intellectual Observer, and he has communicated to the Astronomical Society (*Monthly Notices*, No. 8) some fresh observations on the large spot period of about 56 years, traced by Professor Wolf. Mr. Stewart shows that dates so computed—1836 being the last—correspond approximately to the times when Jupiter and Saturn come to aphelion together, which happens once in 59 years.

New Telescopes by Steinheil.—Mr. De la Rue reports most favourably of a telescope on a new construction by Dr. Steinheil, which was exhibited at his *soirée*, and erroneously described in certain journals as according to the formula of Gauss. It has a 4-inch aperture, and 40 French inches focal length. Dr. Steinheil is, according to *Monthly Notices*, engaged upon another instrument, 6-inch aperture, with object-glass of 3 lenses, and only 30 inches focus.

New Planet.—Mr. Pogson writes to the Astronomer-Royal that his last new planet proves to be identical with *Freia*, but he has since discovered an undoubted new one, to which he assigns the name of Sappho. It is 10·4 mag.

Fresh Human Fossils from Moulin Quignon.—M. Quatrefages has laid before the French Academy a fresh batch of human remains, obtained from Moulin Quignon under circumstances of great precaution against error or fraud. Amongst them is a lower jaw and a skull.

Comets I. and II., 1864.—M. Tempel gives the following particulars respecting the new comet observed by him on the 5th July, and by M. Respighi the next day. Perihelion passage, 7·05 Sep., Marseilles mean time; distance at perihelion, 0·823; Long. at ditto, 289° 37′; R. A., 66° 56′; Inclination, 1° 45′; Retrograde. The second Comet is a small one, seen by Donati in Coma Berenices.

THE SEQUEL OF THE NERVE SUTURE.—We see from *Comptes Rendus* that twelve days after M. Laugier's operation, detailed in our last number, the thread came away, having partially cut through the nerve. Sensation and power of motion had up to this time remained in the fingers, but the inflammation of the nerve gave rise to lancinating pains, with partial insensibility of the palm. Loss of cutaneous sensation was almost complete in the two last phalanges of the index finger, and on the under surface of the thumb and middle finger; but sensation remained on the anterior surface of the first phalanx of the index, on the external half of the palm, and on the outer side of the ring finger. The inflammation subsided in five or six days, and an improved sensibility gradually took place. M. Laugier comments on the singular fact of the inflammation having only affected a portion of the nerve tubes.

FOSSIL STONE IMPLEMENTS IN INDIA.—In the journal of the Asiatic Society of Bengal, No. 1, 1864, will be found an account of the discovery of rude implements made of quartzite, and discovered by Messrs. King and Foote, of the Geological Survey, near Madras.

HERMAPHRODITE BEES.—The *Annals of Natural History*, No. 80, translates some remarks of Von Siebold on hermaphrodite bees, the production of which he thinks consistent with the curious law discovered by Dzierzon, that the queen produces male offspring with eggs that have not been acted upon by the male, while females came from eggs that have been acted upon by spermatozoa. Professor Von Siebold thinks that in the hermaphrodite cases the male semen was insufficient in quantity to effect a complete development of the female sex.

ON ELECTRICAL CHARGES.—The *Proceedings* of the Royal Society, No. 66, contains a paper by Sir W. Snow Harris, verifying the experiments of Le Monnier, Cavendish, and Volta, from which it appeared that bodies do not take up electricity in simple proportion to their surfaces, and giving the formula by which the actual quantity may be calculated. Sir William finds that the quantity which any given surface can receive under *a given intensity*, depends not only on the extent of the surface, but also on its linear boundary. "Thus the linear boundary of 100 square inches of surface under a rectangle 37·5 inches long by 2·66 inches wide, is about 80 inches, whilst the linear boundary of the same 100 square inches of surface under a plate 10 inches square is only 40 inches. Hence the charge of the rectangle is much greater than that of the square, although the surfaces are equal, or nearly so." In plane rectangular surfaces, if the surface be constant, the charge varies with the square root of the boundary, and if the boundary be constant with the square root of the surface. If the surface and boundary both vary, the charge varies with the square root of the surface multiplied with the square root of the boundary. From this law it follows that if when we double the surface we likewise double the boundary, the charge will be double; but if we double the surface without doubling, or with more than doubling the boundary, the charge must be computed by the formula $C = \sqrt{S.B.}$ C meaning charge, S surface, B boundary. Let E stand for intensity, and we have another formula, $E = \frac{1}{S.B.}$

MAGNETIC INDICATIONS AT KEW AND LISBON.—Senor Capello, of the National Observatory, Lisbon, having obtained a set of self-recording magnetic instruments made by Adie upon the Kew pattern, and having resided some time at Kew to familiarize himself with the method of working them, has carried out a series of observations simultaneous with those of Kew, and the results have been published in a series of photolithographs at the expense of the Royal Society. An explanatory paper, issued with the diagrams, observes that a remarkable similarity will be observed on comparing the Horizontal Force Curves, and that rapid changes appear to have occurred in both places at the same time and in the same direction. The Declination Curves exhibit a resemblance but less striking; rapid changes seeming to affect both places similarly and simultaneously. The Vertical Force Curves do not bear much general resemblance, but small and rapid changes appear to have occurred at the same time and in opposite directions.

M. Coste on Infusoria.—This observer denies the assertion of M. Pouchet that ciliated infusoria only appear in solutions after the formation of a pellicle. He finds them frequently developed before the pellicle appears. Glaucoma, Chilodon, and Paramecium he affirms divide themselves without encysting, and Kolpods encyst and then divide. After having multiplied by division in the interior of their cysts, he states that they encyst themselves again, and in this state can remain perfectly dry for an indefinite time, and resume activity when moistened. Such encysted Kolpods he finds to abound in the fine dust that may be shaken from dry hay.—*Comptes Rendus.*

Do Bacteriums Cause Disease?—In a former number we gave an account of M. Davaine's experiments, in which death followed the inoculation of healthy animals with a few drops of blood containing bacteriums, and taken from other animals suffering under spleen disease. MM. Leplat and Jaillard have communicated counter experiments to the French Academy. They obtained bacteriums from vegetable and animal solutions, and introduced them into the circulation of animals without producing any evil effects. From this they conclude that in M. Davaine's experiments it was the diseased blood and not the bacteriums that caused the mischief. The question is, however, far from settled by the new experiments, and their authors are by no means entitled to assume that, because vibrions are much alike in appearance, their properties must be the same. It would be more in accordance with observation to state exactly the contrary, and affirm that very similar bodies of this kind are connected with different kinds of fermentation and putrefaction.

Curious Physical Experiment.—An interesting experiment, which, though not new, is not generally known, may be performed as follows:—Roll up a large card into a tube a quarter of an inch in diameter, and make the joint tight by a little sealing-wax. Then cut a disc of card two inches in diameter, make a hole through its centre exactly big enough to admit the tube. Sealing-wax the card disc on to the top of the tube so as to form a flange, taking care not to let the tube project above the surface of the disc. Cut another card disc of the same diameter, and lay it on the former, holding the tube quite upright with the disc uppermost. Blow gently through the tube, and the loose disc will be thrown off the flange. Replace it, and blow with great vehemence. The disc will not then be thrown off, but will remain close to the flange vibrating strongly. The loose disc may then be placed on the table, and the tube with the flange downwards held very near it. On blowing violently the loose disc will spring up towards the flange and vibrate as before.

Earthquake at Lewes.—On Sunday morning, August 21st, about 1·27 A.M. an earthquake shock was felt at Lewes, and in some other parts of Sussex. Some persons heard a noise as of an explosion, and objects in their hands rattled, while others were quite unconscious that anything of the sort had occurred.

New Telescope Stand.—The Rev. E. L. Berthon has devised a singularly convenient and economical telescope stand, which has an elegant appearance, and only costs a small fraction of the price charged for the ordinary cross tripod stands, which it excels in every way. The vertical movement is given by a screw, which works more pleasantly than the ordinary rack, and the horizontal movement is obtained by making the base of the stand rest on three rollers, so placed that a slight pressure with the finger communicates a circular motion in either direction. An important novelty in the vertical movement is an ingenious method by which it can be instantaneously altered from a quick to a slow motion, or *vice versa*, as occasion requires. As a table stand for seaside use, and for astronomical purposes, this invention is superior to anything previously produced, while it has the further recommendation of great portability. A ten-inch telescope with the Berthon stand can be packed in a case not so long, and little broader than a stout umbrella. Messrs. Horne and Thornthwaite have the credit of bringing this excellent invention before the public.

THE MAY FLY.—GREEN AND GREY DRAKE.
(*Ephemera vulgata.*)